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JOURNAL OF THE
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The New Journal Format

In June, 1946, the first post-war issue of the *Journal* appeared with a grey cover and a page size of 9 in. by 6 in. Over the subsequent twelve years that size was retained and, while the tone of the cover mellowed slightly, it remained the familiar grey uniform that identified the publication to many thousands of readers throughout the world. It is with a certain degree of sadness that we must now record the passing of the old format under which the *Journal* has become an established part of the literature of astronautics and its replacement by a larger, double-column-page format with a blue cover.

The increase in size of the *Journal* page has been dictated by the need to expand the publication in order to cope with the rapidly increasing flow of good material which is now being written in the field of astronautics. The first few issues will not actually be significantly larger, in terms of the volume of the contents, than some of the recent issues of the old size. However, in the near future, it is proposed either to publish the *Journal* monthly, or else to greatly increase the number of pages of the bimonthly issues. The Council considered that an increase in page size would facilitate this expansion and that uniformity in size of the *Journal* and *Spaceflight* would have many advantages.

The primary purpose of the *B.I.S. Journal* is to provide a medium for the publication of original technical papers and new ideas that may contribute to the growing science of astronautics. This policy has already been pursued with some success during the post-war years and the *Journal* has established for itself a firm place in the literature of spaceflight. The new format and subsequent expansion should enable us to maintain that position and, thereby, play a useful part in the advance of astronautical science.

With this issue the Council and Officers of the Society take pleasure in presenting the *Journal* in its new form. We hope that it will please our many readers and members and provide them with the information on astronautical matters which they demand.

A. F. Shepherd.

Chairman of Council

METHODS FOR PREDICTING THE ORBITS OF NEAR EARTH-SATELLITES*

By D. G. KING-HELE,† M.A., and Mrs. D. M. C. WALKER‡

(Communication from The Royal Aircraft Establishment)

ABSTRACT

Methods are described for predicting the times and positions of the daily transits of a satellite, and the geometry of its orbit. The methods depend upon maintaining an accurate record of the period of revolution, from which the other orbital elements are deduced theoretically.

I. INTRODUCTION

Most of the artificial Earth satellites so far launched have been appreciably retarded by air drag near perigee, and their periods of revolution have decreased quite rapidly. The daily decrease in period would change in a regular and predictable manner if the density of the atmosphere at a given height were constant from day to day and if the effective cross-sectional area of the satellite did not vary. In practice, however, with all the satellites so far launched, the daily decrease in period has varied irregularly, and has departed from the value predicted by theory by up to 30%, as Fig. 1 shows. Most of these irregularities are thought to be caused by day-to-day variations in atmospheric density, for the "weather" aloft is probably even more variable than it is here below. But some of the irregularities may be due to changes in the electrical state of the atmosphere or in the effective

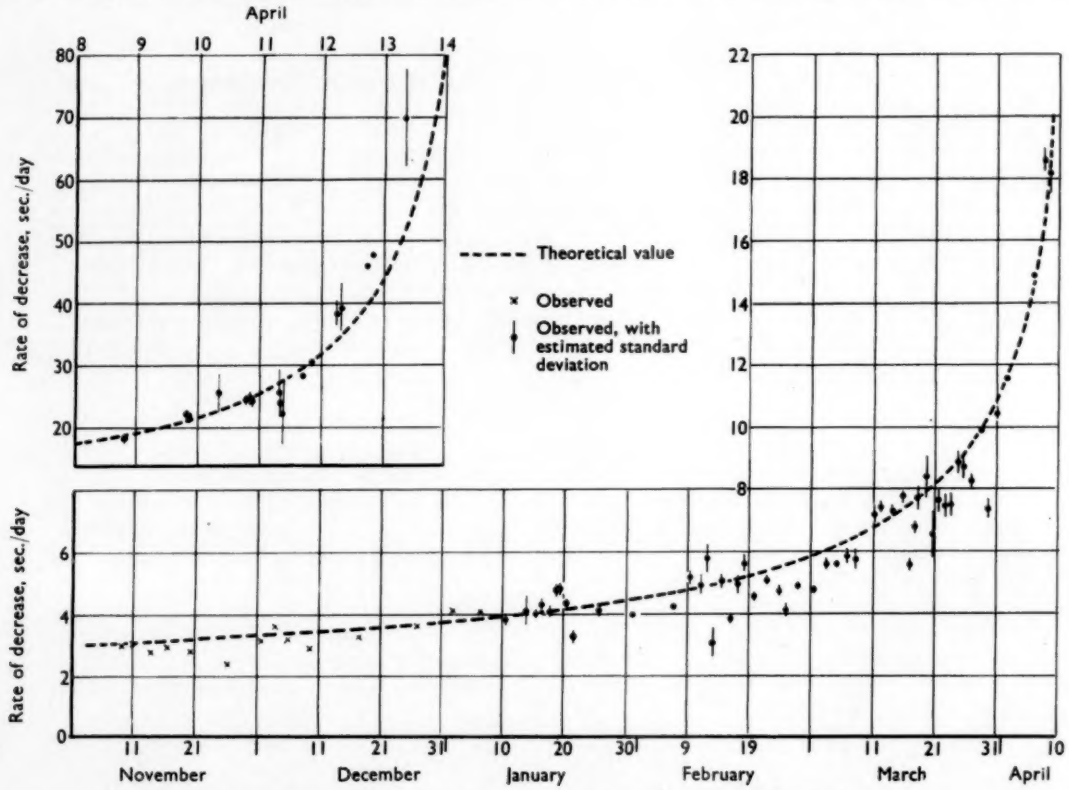

FIG. 1. Rate of change of period for *Sputnik 2* between November, 1957 and April, 1958

* Manuscript received 1 November, 1958.

† Principal Scientific Officer, Royal Aircraft Establishment, Farnborough, Hants.

‡ Assistant Experimental Officer, Royal Aircraft Establishment, Farnborough, Hants.

cross-sectional area of the satellites: for example, a satellite may stop tumbling and fly like an arrow.

Because of these irregularities it has not been possible to make confident predictions for the times of transit of short-lived satellites, such as *Sputniks 1* and *2* and the rocket of *Sputnik 3*, for more than about a week in advance. Rapid and simple methods were therefore needed, so that as little time as possible was taken up with calculation. This article describes the methods developed during 1958 while the authors were responsible for the prediction service in Britain. By using these methods, for which only simple computation is required, the times and positions of transits each day can be predicted, and a record can be obtained of the geometry of the satellite's orbit throughout its lifetime. Visual observations with an accuracy of $\pm 1°$ in direction and ± 2 sec. in time suffice to determine the geometry of the orbit with good accuracy, provided an initial position and height of perigee, and the inclination of the orbit to the equator are known.

II. METHOD OF PRESENTING PREDICTIONS

It is helpful to begin by describing the finished product of the prediction process. This consisted of:

(1) A map of part of the northern hemisphere, Fig. 2.

(2) A track diagram, of which Fig. 3 is an example, giving the path of the satellite over the Earth's surface, its position at time intervals of one minute from apex (the point of maximum latitude north) and its height at these times. The track diagram, printed on transparent paper, was fixed to the map by a drawing pin through the north pole, and was thus free to rotate to the appropriate longitude.

(3) Setting data, consisting of a predicted apex time, T_N, and apex longitude L_N for every transit near Britain, and an indication of visibility. Fig. 4 is an example, though it was usual to issue predictions for about 10 days ahead rather than 4.

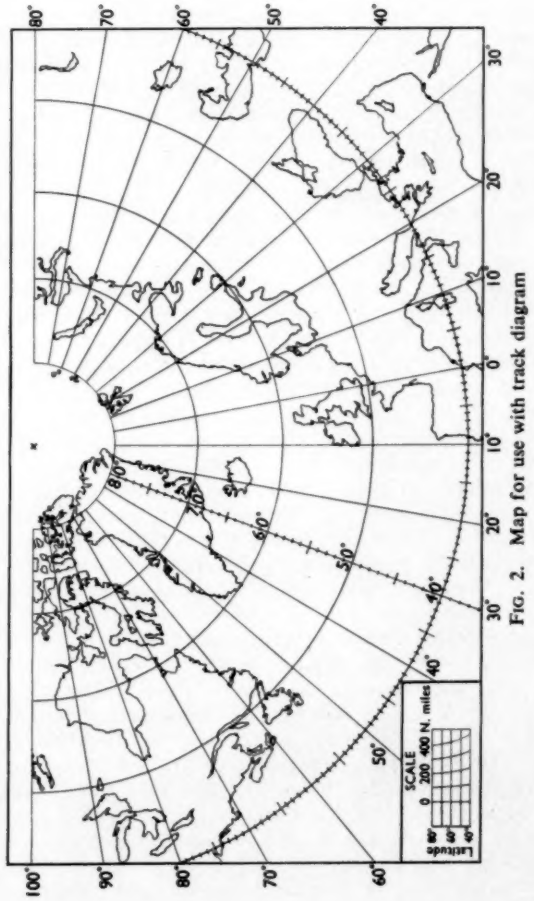

FIG. 2. Map for use with track diagram

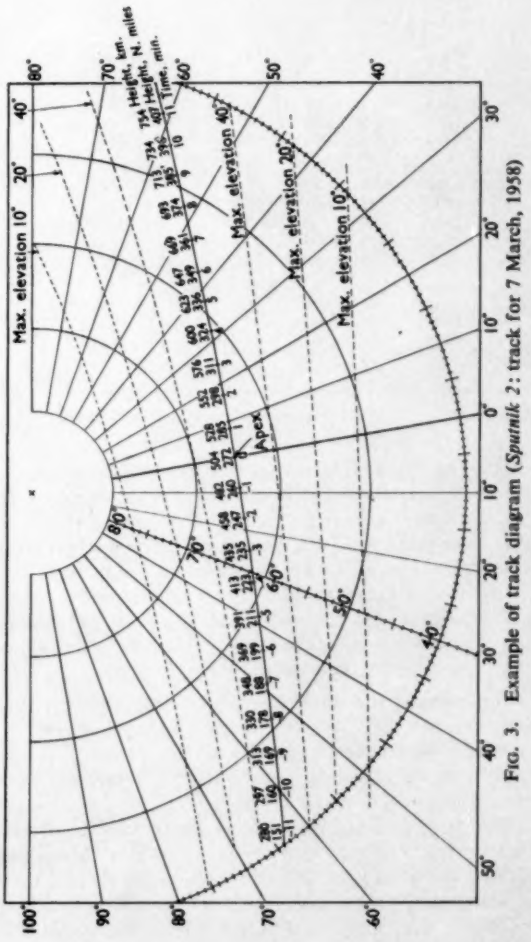

FIG. 3. Example of track diagram (*Sputnik 2*: track for 7 March, 1958)

Setting Data for Track Diagram of Sputnik 2

N in the first column is the revolution number.

T_N in the second column is the time at which the satellite passes through the apex (the point of maximum latitude). Times from apex are indicated on the track diagram.

L_N in the third column is the longitude of apex, and the track diagram should be rotated until the heavy line, marked 0°, is at longitude L_N.

The last column indicates visibility:

"Yes" means that the satellite is illuminated by sunlight, and that in some or all of Britain the sky is dark enough for the satellite to be seen.

"No" means the satellite cannot be seen, either because it is in the Earth's shadow, or because the sky is too bright.

If the satellite goes into eclipse or emerges from eclipse between longitude 20W and 20E, the longitude of eclipse is given in the "Vis" column.

	T_N				
	March				
N	d	h	m	L_N	Vis
1796	7	00	00	51E	No
1797	7	01	36	27E	No
1798	7	03	12	3E	3W
1799	7	04	48	21W	Yes
1800	7	06	24	46W	No
1811	7	23	58	48E	No
1812	8	01	34	23E	No
1813	8	03	10	1W	1E
1814	8	04	46	25W	Yes
1815	8	06	22	49W	No
1826	8	23	55	44E	No
1827	9	01	31	20E	No
1828	9	03	07	4W	5E
1829	9	04	42	28W	19W
1830	9	06	18	53W	No
1841	9	23	50	41E	No
1842	10	01	26	17E	No
1843	10	03	01	7W	8E
1844	10	04	37	31W	16W
1845	10	06	13	55W	No

FIG. 4. Example of setting data

The track diagram had to be rotated until the thick line marked 0° coincided with longitude L_N on the map underneath. This then defined the satellite track: the maximum elevation from any observing point was indicated by the broken lines on the diagram, and the time of transit at any latitude was found by adding to (or subtracting from) the predicted apex time the number of minutes marked on the track diagram.

Fig. 5 shows the track diagram in use, set for revolution number 1829 on 9 March. From London the satellite would have appeared highest in the sky to the N.N.E. at an elevation of about 25°, 5 minutes after apex, *i.e.*, at 04.47 G.M.T.

The heights and times from apex change slowly because the major axis of the orbit rotates slowly and because the orbit gradually contracts under the influence of air drag. Track diagrams were issued once a fortnight for *Sputniks 2* and *3*, for which the major axis rotated at less than ½° per day. In a fortnight the heights usually changed by less than 15 miles and the spacing of the 1-minute time marks did not change perceptibly. The lists of setting data were usually issued once a week, though they often had to be amended because of the irregularities in the rate of change of period.

As the form in which the predictions were issued suggests, there are two main tasks in prediction, which are nearly but not quite independent,

(1) predicting the geometry of the orbit and the time taken by the satellite to travel from apex to any other point in the orbit, to construct the track diagram, and

(2) predicting the time at apex and its position relative to the Earth, to obtain the setting data.

For both these tasks the first essential is to record the past history of the period of revolution T of the satellite, and the method of obtaining T from visual or other observations will now be described.

III. DETERMINING THE PERIOD OF REVOLUTION

It is first necessary to decide which of the several possible definitions of period should be used. The difficulty arises because the satellite does not move in a true ellipse, but in an orbit whose major axis rotates as a result of the Earth's oblateness. The *anomalistic period*, the time which elapses between two successive closest approaches to the Earth's centre, is in some ways the most logical period to use: but it is slightly unsatisfying because the satellite's angular travel during one anomalistic period differs appreciably from 360°; and it has a serious practical disadvantage, because the point of closest approach is very difficult to determine accurately. It is the *nodal period* which proves to be most convenient for prediction purposes. The nodal period is defined, strictly, as the time between successive northward crossings of the Earth's equatorial plane, or, more loosely, as the time between successive northward crossings of any line of latitude. The nodal period has one slight disadvantage: it is equal to the anomalistic period plus (or minus) the time which the satellite takes to cover the small angle through which the major axis rotates during one revolution of the satellite, and this time will be shorter when the satellite is near perigee than when it is near apogee. The nodal period thus differs slightly according to the line of latitude chosen, and whatever the line of latitude chosen (even if it is the strictly correct one, the equator), the nodal period suffers a slow oscillation as the perigee moves round. For *Sputniks 1*, *2* and *3* with their slow movement of perigee, about 0·03° per revolution, the oscillation was almost negligible, having an amplitude less than 0·005 minutes and a period greater than 600 days; but for some of the U.S. satellites, with their faster perigee movement of up to 0·5° per revolution, the effect is quite pronounced. For satellites which pass over Britain, the most convenient point to choose for defining the nodal period is the apex,

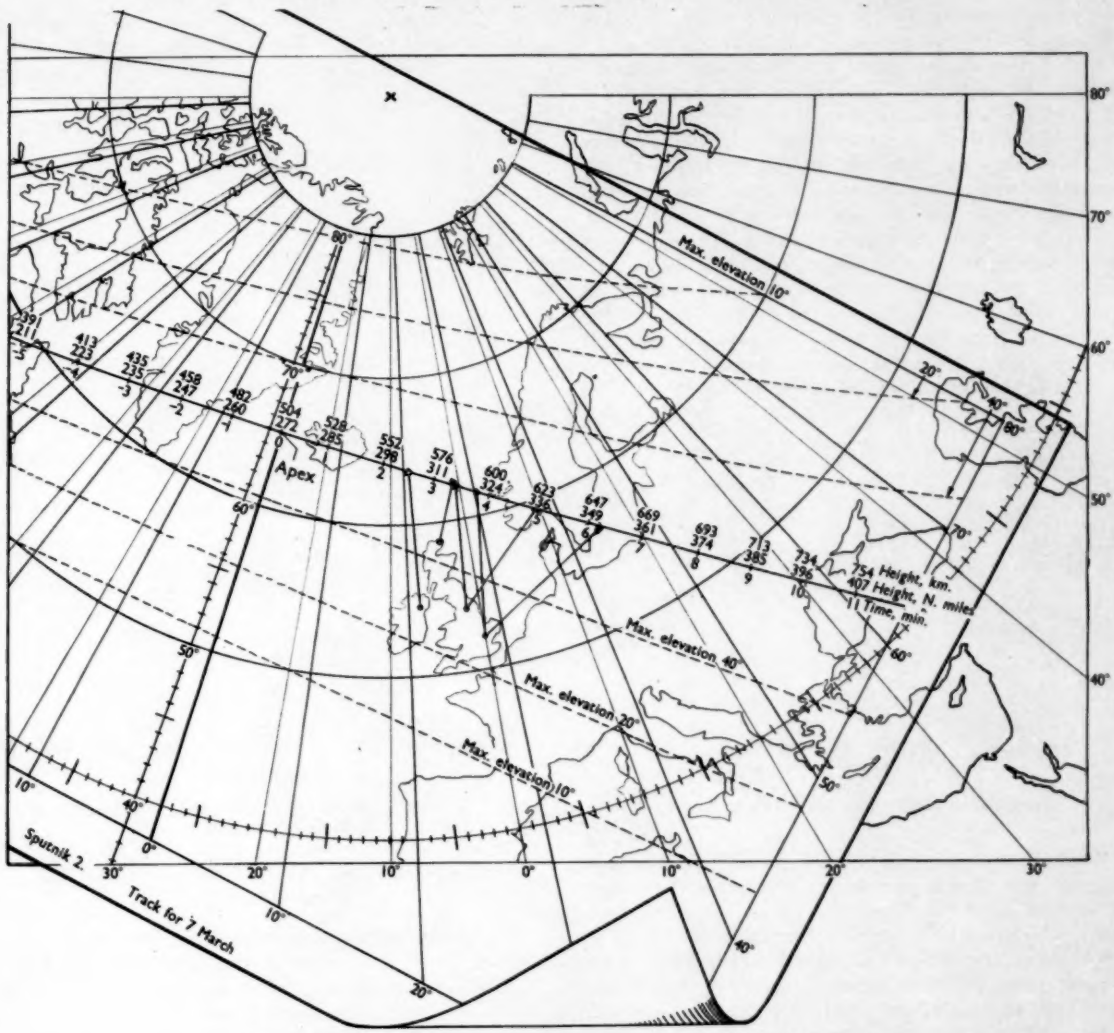

FIG. 5. Example of track diagram in use, set for revolution No. 1829

the point of maximum latitude north, and all observations received for a particular transit were used to obtain a best estimate of apex time.

Before the observations could be utilized for this purpose they had to be expressed in terms of azimuth (measured from north through east) and altitude (the angle above the horizontal). Many observations, such as those made with theodolites, were received in this altitude-azimuth form, but most visual observations were made relative to the stars and given in the form: "$\frac{7}{10}$ of the way between α and β Ursae Majoris at 19 hr. 37 min. 53 sec." This type of observation was accurate enough for prediction purposes, since most regular visual observers achieved an accuracy of better than $\pm 1°$ in position and better than ± 2 sec. in time.

Observed positions relative to the stars were first converted to right ascension and declination, Norton's Star Atlas being found adequate. Then the right ascension (R.A.) and time were used to obtain the hour angle, which is equal to S.T. minus R.A., where S.T. equals sidereal time of observation plus 4 μ minutes, μ being the longitude of the observer east in degrees. Finally, hour angle h and declination δ were converted to altitude a and azimuth A using tables[1] or the formulae[2]:

$$\sin a = \sin\delta \sin\lambda + \cos\delta \cos\lambda \cos h, \quad \ldots \quad (1)$$

$$\sin A = \cos\delta \sin h \sec a, \quad \ldots \quad (2)$$

where λ is the observer's latitude. The calculated value of altitude should, strictly, be corrected for atmospheric

refraction, but since the correction is less than 0·2° for altitudes above 5° it was usually ignored.

Each observation, when converted into terms of altitude and azimuth, can be plotted as a point on the map of Fig. 2, by the following procedure. The track diagram is set to the predicted value of L_N and a line is drawn on the map from the observer's position at the correct azimuth. The point where this line cuts the track gives the height of the satellite at the time of observation. Since its angle of elevation is known, the distance along the Earth's surface from the observer to the point directly beneath the satellite can readily be found, most simply from a diagram like Fig. 6. By marking off this distance along the azimuth line, the point on the Earth's surface directly beneath the satellite at the time of observation can be plotted on the map.

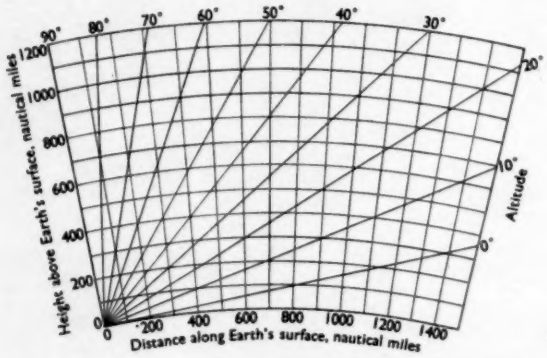

FIG. 6. Diagram to determine distance along Earth's surface

When several such points had been plotted, the track diagram was re-set to pass as nearly as possible through all of them, as in Fig. 5, and this gave a corrected value of L_N, which may differ slightly from the predicted value (though the difference is imperceptible in Fig. 5). With the track diagram still in position, as in Fig. 5, the time from apex at each of the observed points was read off, with an error of less than 2 seconds, on the scale of the original diagram. Adding (or subtracting) these times-from-apex, each observation yielded an estimate of apex time, and the mean of these values (excluding any that differed by more than, say, 10 sec. from the mean) was taken as the best estimate of apex time. It is believed that the apex times determined in this way were usually in error by less than 3 sec.

By these methods values of apex time T_N were obtained for every observed transit of the satellites. The period of revolution was then found with adequate accuracy by taking apex times on successive days and dividing the time interval by the number of revolutions intervening. For example, if T_N = 21 hr. 15 min. 43 sec. $\pm$3 sec. on day 1, and T_{N+14} = 21 hr. 23 min. 52 sec. $\pm$3 sec. on day 2, the period T at 09 hr. 20 min. on day 2 (half-way between T_N and T_{N+14}) is 103·439 $\pm$ 0·005 minutes. Thus T could be determined with an accuracy of 1 part in 20,000, and an accurate record of past values of T was obtained. The variation of T for *Sputnik 2*, and for *Sputnik 3* and its rocket up to 22 October, 1958, is shown in Fig. 7.

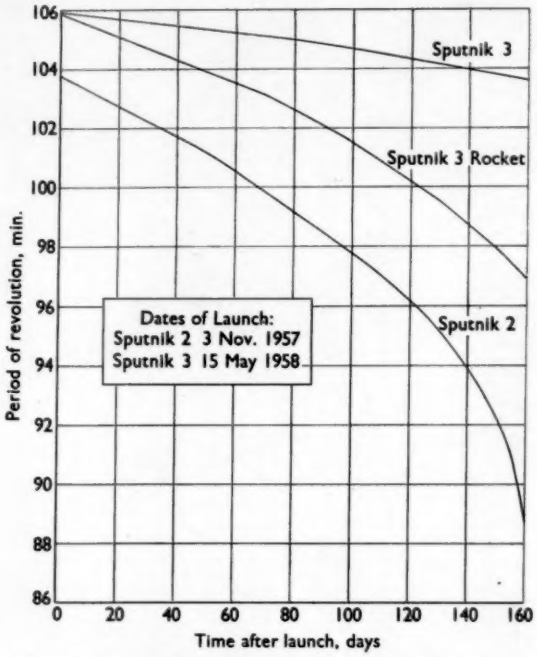

FIG. 7. Variation of period of revolution with time for *Sputnik 2*, *Sputnik 3*, and its rocket

IV. PREDICTING APEX TIME AND APEX LONGITUDE

To predict apex time and longitude, the record of past values of T has to be extrapolated into the future. There are various ways of doing this, some scientific, some intuitive. The most scientific way is to use the theory for the variation of period with time[3]; another popular method is to fit a polynomial to the past values. But neither of these methods can pretend to cope with irregularities like those in Fig. 1. Predicting the future values of T is at present, it must be admitted, more of an art than a science, and is likely to remain so until the irregularities can be fully explained and forecast. We found it most satisfactory to extend the period-versus-time curve by eye, relying on experience of its past peculiarities to try to anticipate future ones. For example, as Fig. 7 shows, the period/time curve, contrary to both theory and fitted polynomials, had a strong tendency to take the form of a series of straight lines, and as soon as a "corner" was recognized it was usually found best to continue the curve as a straight line for a few weeks.

To illustrate the difficulties of prediction, an example chosen at random during the life-time of *Sputnik 2*, is

given in Fig. 8. The full line shows the variation of T up to March 4.0, the triangles indicating observed values. The broken lines show the future values, as

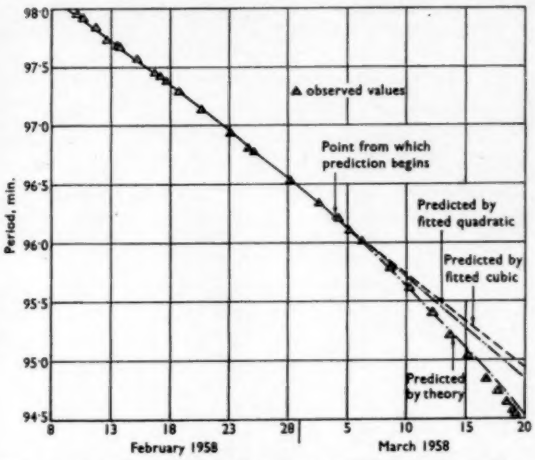

FIG. 8. Variation of the period of revolution of *Sputnik 2*, showing predicted and observed values

predicted by (1) a cubic in t fitted at four weekly intervals, (2) a quadratic in t fitted at three weekly intervals, and (3) the simplest form of theory[3]

$$\frac{T}{T_0} = 1 - \frac{3}{2} e_0 \left(1 - \sqrt{1 - \frac{t}{t_L}}\right), \quad .. \quad (3)$$

where e_0 is the initial eccentricity, T_0 the initial period, and t_L the life-time, determined by substituting in (3) the latest observed value of T. It will be seen from Fig. 8 that, by the end of a fortnight, the predicted values of T given by (1), (2) and (3) are in error by 0·43, 0·34 and 0·09 minutes respectively, corresponding to errors in predicted apex time on 18 March of about 45, 35 and 10 minutes respectively.

Three comments should be made on these results. First, it is only by chance that theory is so much better than the fitted polynomials: there were times when it was just as bad or worse. Second, the quadratic gives better results than the cubic, as might be expected: using high-order polynomials is a policy of desperation, and Fig. 9 provides a useful warning. Third, it will be noticed that the values of T assumed in the actual predictions have been tactfully omitted: in fact the predictions made at the beginning of March by extrapolating the curve by eye were in error by 7 minutes after a fortnight—not much better than the theory.

The predicted values of T, obtained in one of the ways mentioned above, were used to predict apex time T_N and apex longitude L_N, starting from the last reliable observational values of these quantities. It was found most convenient to work in terms of ϵ, the number of minutes by which n revolutions fall short of 24 hours, where n is chosen so that $|\epsilon|$ is as small as possible. With T expressed in minutes, ϵ is given by

$$\epsilon = 1440 - n\,T \quad .. \quad .. \quad (4)$$

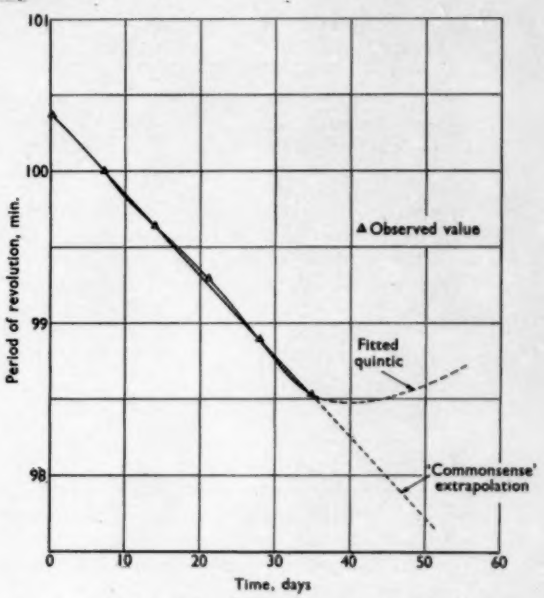

FIG. 9 The dangers of extrapolating the period/time curve with a high-order polynomial

If the period of revolution of the satellite is less than 120 minutes, n will be 12, 13, 14, 15 or 16 (for T can never be less than 87 min.), and $|\epsilon|$ will never exceed 60 min. Since $|\epsilon|$ is a small fraction of a day it proves possible to express the formulae for determining T_N and L_N as linear functions of ϵ, with acceptable accuracy.

If T_{N_1} on day 1 is the latest observed value of T_N, the apex time n revolutions later on day 2 is given by

$$T_{N_1+n} = T_{N_1} - \epsilon, \quad .. \quad .. \quad (5)$$

the value of T taken in (4) being that appropriate to a time half-way between time T_{N_1} on day 1 and T_{N_1+n} on day 2, $T = \bar{T}$ say. It is usually necessary to predict a few transits before and after the most convenient one, and apex time is found by adding or subtracting T the appropriate number of times, *i.e.*,

$$T_{N_1+n-r} = T_{N_1+n} - rT \quad .. \quad .. \quad (6)$$

Strictly the value of T in (6) should be that appropriate to time $T_{N_1+n-\frac{1}{2}r}$ on day 2, but, except near the end of a satellite's life, little error will be incurred by using $\bar{T}$ (if T changed by 2 sec./day the error in T_{N_1+n-8} through using $\bar{T}$ instead of the correct T in (6) would be less than 4 sec., and predictions were usually rounded off to the nearest minute).

In predicting apex longitude L_N, the theoretical formula for the rate of rotation of the orbital plane was used. The Earth's gravitational potential U may be expressed as

$$U = gR\left\{\frac{R}{r} + J\frac{R^2}{r^3}\left(\frac{1}{3} - \cos^2\theta\right) + \right.$$
$$\left. + \frac{D}{35}\frac{R^5}{r^5}(35\cos^4\theta - 30\cos^2\theta + 3)\right\}, \quad (7)$$

where R is the Earth's equatorial radius (3441·69 n. miles),
r is the distance from the Earth's centre,
θ is the angle between the Earth's axis and the radius vector,
J and D are constants, whose values are discussed later.

The rotation of the orbital plane is in the direction opposite to the satellite's motion, so that the plane rotates from east to west for all satellites launched up to November, 1958, and the rate of rotation is found[4] to be

$$\dot{\Omega} = \sqrt{\frac{g}{R}}\left(\frac{R}{a}\right)^{1\cdot5}\left(\frac{R}{\bar{r}}\right)^{2}\cos\alpha\left[J + \frac{J^2R^2}{\bar{r}^2}\left(\frac{19}{12}\sin^2\alpha - 1\right) - \right.$$
$$\left. - \frac{3DR^2}{14\bar{r}^2}(7\sin^2\alpha - 4) + O(J^2e)\right], \quad .. \quad (8)$$

where a is the semi major axis of the orbit,
$\bar{r}$ is the (harmonic) mean distance of the satellite from the Earth's centre, equal to $a(1 - e^2)$,
α is the inclination of the orbital plane to the equator.

In the pre-Sputnik era the values of the coefficients J and D were determined from geodetic data, and the recommended values[5] were $J = 1{\cdot}637 \times 10^{-3}$ and $D = 10{\cdot}6 \times 10^{-6}$.

Comparison of theory and observation for *Sputnik 2* suggests[6] that these values are in need of revision, and agreement with observation can be obtained by taking $J = 1{\cdot}626 \times 10^{-3}$ and leaving D unchanged at $10{\cdot}6 \times 10^{-6}$ (other pairs of values are possible, but for prediction purposes any of the possible pairs can be used).

For rapid prediction, equation (8) must be simplified, but it is difficult to specify the errors in L_N that should be allowed to accrue as a result of the simplification. Since T_N will often be in error by 1 minute it is not usually worth striving for an accuracy in L_N of better than 0·25°, the change in L_N during one minute as a result of the Earth's rotation. If, as is desirable, good observational values of L_N are obtained at least once a week, the predicted orientation of the orbital plane in space should not be in error by more than 0·25° after a week. Thus $\dot{\Omega}$ would be required correct to 0·035° per day or about 1% for a satellite like *Sputniks 1–3*. It will be assumed here, in simplifying (8), that 1% accuracy will suffice, though sometimes, *e.g.*, when a satellite is not suitably placed for observation for a month, a more accurate form may be needed.

To simplify equation (8) we can begin by neglecting the terms in J^2 and D, which never contribute more than 0·4% of the total $\dot{\Omega}$, and for $\alpha = 65°$ contribute only 0·15%. Equation (8) then becomes, on writing $\bar{r} = a(1 - e^2)$,

$$\dot{\Omega} = J\sqrt{\frac{g}{R}}\left(\frac{R}{a}\right)^{3\cdot5}(1 - e^2)^{-2}\cos\alpha \quad .. \quad (9)$$

This must now be expressed in terms of ϵ. The period of revolution T and the semi major axis a are related[4] by the equation:

$$T = 2\pi\sqrt{\frac{a^3}{gR^2}}\left\{1 - \frac{JR^2}{a^2}\frac{7\cos^2\alpha - 1}{4}\right\}, \quad (10)$$

while, from (4), T in seconds is given by

$$T = 60\,(1440 - \epsilon)/n \quad .. \quad .. \quad (11)$$

The maximum value of the J term in (10) is 0·0024, when $\alpha = 0$ and $a = R$, and it follows that the J term can be ignored when determining a from T, with error <0·2%. Making this approximation, (10) and (11) give

$$\frac{R}{a} = \left(\frac{n\pi}{43200}\right)^{2/3}\left(\frac{R}{g}\right)^{1/3}\left\{1 - \frac{\epsilon}{1440}\right\}^{-2/3} \quad .. \quad (12)$$

Substituting (12) in (9), we have

$$\dot{\Omega} = J\left(\frac{n\pi}{43200}\right)^{7/3}\left(\frac{R}{g}\right)^{2/3}\left(1 - \frac{\epsilon}{1440}\right)^{-7/3}(1 - e^2)^{-2}\cos\alpha$$
$$.. \quad .. \quad (13)$$
$$= 0{\cdot}01335\, n^{7/3}\left(1 - \frac{\epsilon}{1440}\right)^{-7/3}(1 - e^2)^{-2}\cos\alpha \text{ deg./day},$$
$$.. \quad .. \quad (14)$$

on inserting numerical values.

During n revolutions, which take $\left(1 - \frac{\epsilon}{1440}\right)$ days, the orbital plane rotates, relative to the stars, through an angle $\dot{\Omega}\left(1 - \frac{\epsilon}{1440}\right)$ degrees to the west. During the same time-interval the Earth, which rotates through 360·986 degrees relative to the stars in 1 day, turns through $360{\cdot}986\left(1 - \frac{\epsilon}{1440}\right)$ degrees to the east. Apex longitude on the $(N + n)$th orbit is therefore west of apex longitude on the Nth orbit by

$$f = 360{\cdot}986\left(1 - \frac{\epsilon}{1440}\right) -$$
$$- 360 + \dot{\Omega}\left(1 - \frac{\epsilon}{1440}\right) \text{ degrees} \quad (15)$$

(The $\dot{\Omega}$ term would be subtracted instead of added if the satellite were going from east to west). On substituting $\dot{\Omega}$ from (14), equation (15) becomes

$$f = 0{\cdot}986 - 0{\cdot}2507\epsilon + 0{\cdot}01335\, n^{7/3}\left(1 - \frac{\epsilon}{1440}\right)^{-4/3} \times$$
$$\times (1 - e^2)^{-2}\cos\alpha \text{ degrees} \quad (16)$$

The variation of f with ϵ for various values of n and α is shown in Fig. 10, for $e = 0$. The change in f caused by increasing e to 0·1 would be imperceptible on the scale of Fig. 10.

For practical purposes equation (16) requires to be further simplified. The term $\left(1 - \frac{\epsilon}{1440}\right)^{-4/3}$ may be taken as $\left(1 + \frac{\epsilon}{1080}\right)$ with error <0·3%. And since e is

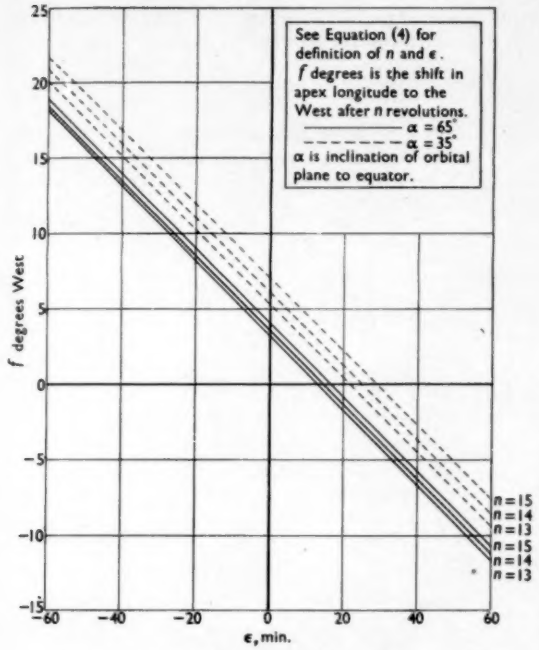
See Equation (4) for definition of n and ϵ.
f degrees is the shift in apex longitude to the West after n revolutions.
α = 65°
α = 35°
α is inclination of orbital plane to equator.
f degrees West
ϵ, min.
n = 15
n = 14
n = 13
n = 15
n = 14
n = 13

FIG. 10. Variation of f with ϵ when $e = 0$

usually small, $(1 - e^2)^{-2}$ may be taken as 1·005 for $0 < e < 0{\cdot}07$; 1·015 for $0{\cdot}07 < e < 0{\cdot}1$; and so on, all with error $<0{\cdot}5\%$.

The appropriate final form for f is then

$$f = 0{\cdot}986 - 0{\cdot}2507\,\epsilon + K\left(1 + \frac{\epsilon}{1080}\right)\cos\alpha \quad .. \quad (17)$$

where $K = 0{\cdot}01335\,n^{7/3}\,(1 - e^2)^{-2}$ may be taken as having the values in Table I, when $e < 0{\cdot}14$. For larger values of e it is not advisable to seek an approximation for $(1 - e^2)^{-2}$.

TABLE I.—*Values of the Coefficient K in Equation* (17)

e \ n	12	13	14	15	16
$0 < e < 0{\cdot}07$	4·42	5·33	6·34	7·44	8·65
$0{\cdot}07 < e < 0{\cdot}1$	4·47	5·38	6·40	7·52	8·74
$0{\cdot}1 < e < 0{\cdot}12$	4·51	5·44	6·46	7·59	8·82
$0{\cdot}12 < e < 0{\cdot}14$	4·55	5·49	6·53	7·66	8·91

The same value of K usually applies for a month or more, because e remains within the same range for some months and n changes infrequently. With *Sputnik 2*, for example, the appropriate values of K were 6·40 from launch (3 November) till 22 January, 7·52 until 30 January, 7·44 until 31 March, and 8·65 until the end of its life. The change in α during the lifetime of a satellite will usually be very small, of order 0·1°, and for prediction it will often be adequate to take α as constant. For prediction, then, f reduces to the simple form

$$f = A - B\epsilon, \quad .. \quad .. \quad .. \quad (18)$$

with the coefficients A and B changing occasionally, as n changes or e enters a new range.

If L_{N_1} on day 1 is the last observed value of the apex longitude L_N (longitude west being taken positive) the value n revolutions later is

$$L_{N_1+n} = L_{N_1} + f, \quad .. \quad .. \quad (19)$$

where f is given by (17). The separation in longitude between successive transits, F, is given by

$$F = \frac{360 + f}{n} \text{ degrees}, \quad .. \quad .. \quad (20)$$

and consecutive values of L_N are found by adding F. Thus

$$L_{N_1+n+r} = L_{N_1+n} + r\,F \quad .. \quad .. \quad (21)$$

This completes the procedure for predicting T_N and L_N.

The preparation of a track diagram requires

(1) prediction of the orbital elements two to three weeks ahead, and
(2) calculation of the track over the Earth, given the orbital elements.

These two processes will now be described.

V. PREDICTING THE ORBITAL ELEMENTS

To calculate the track diagram, five of the orbital elements must be known:

(1) the period of revolution, T;
(2) the semi major axis, a;
(3) the eccentricity e, or perigee distance, r_p;
(4) the angle from apex to perigee, β;
(5) the inclination of the orbit to the equator, α.

Any method of determining the orbital elements directly from observations (*e.g.*, that described in References 7 and 8) is bound to involve lengthy computation, and, even if the computational facilities are available, there is no guarantee that up-to-date observations of the required accuracy will be received. Also, even if accurate recent values for the orbital elements are obtained, errors will still arise in extrapolating them into the future.

For prediction purposes a simple analytical method is obviously desirable. Fortunately the orbital elements can be found with adequate accuracy from theory, provided that information is available about certain initial values, namely the initial inclination of the orbit to the equator α_0, the initial eccentricity e_0, and the initial position of perigee, *i.e.*, the value of β_0.

The first of the orbital elements, T, was predicted by extrapolating the record of past values, by one of the methods already described. The value of a was then

found, either directly from (10) using an iterative process, or by expressing (10) as

$$a = 178{\cdot}75\{T^{2/3} + 0{\cdot}100\,T^{-2/3}\,(7\cos^2\alpha - 1)\} \text{ n. miles} \quad (22),$$

where T is in minutes. The predicted value of T was often in error, but errors which might be serious in predicting apex time T_N do not greatly affect the orbital elements. An error of 0·2 min. in the predicted value of T at the end of a fortnight, which would produce an error of 20 min. in T_N at the end of a fortnight, would lead to an error of only about 5 n.miles in the semi major axis a.

To find the third orbital element, the eccentricity, we used the theoretical relation between T and e for an orbit contracting under the influence of air drag[3]:

$$\frac{T}{T_0} = \left(\frac{1-e_0}{1-e}\right)^{3/2}\left\{1 - \frac{3H}{4a_0}\ln\frac{(1+e)\,e_0}{(1+e_0)\,e}\right\} \quad .. \quad (23).$$

In deriving this expression, air density ρ is assumed to vary exponentially with height y, and H is the "scale height": $\rho \propto \exp(-y/H)$. For satellites with perigee height near 120 n. miles, like *Sputniks 1–3*, H is thought to be near 30 n. miles, so that $H/a_0 \simeq 0{\cdot}008$: T/T_0 is plotted against e for various e_0 and H/a_0 in Fig. 11, which shows that the value of H/a_0 need not be accurately known until near the end of the satellite's life. The eccentricity can be calculated by numerical solution of (23), but in practice an indirect graphical method was found more convenient. The change in perigee distance during a satellite's life, $r_{p0} - r_p$, was plotted against e, as in Fig. 12, using the formula[3]:

$$r_p = r_{p0} - \frac{H}{2}\ln\frac{(1+e)\,e_0}{(1+e_0)\,e} \quad .. \quad .. \quad (24)$$

e was then read off roughly from Fig. 11, r_p was obtained from Fig. 12 or equation (24) (r_{p0} being known) and e was calculated from

$$e = 1 - \frac{r_p}{a} \quad .. \quad .. \quad .. \quad (25)$$

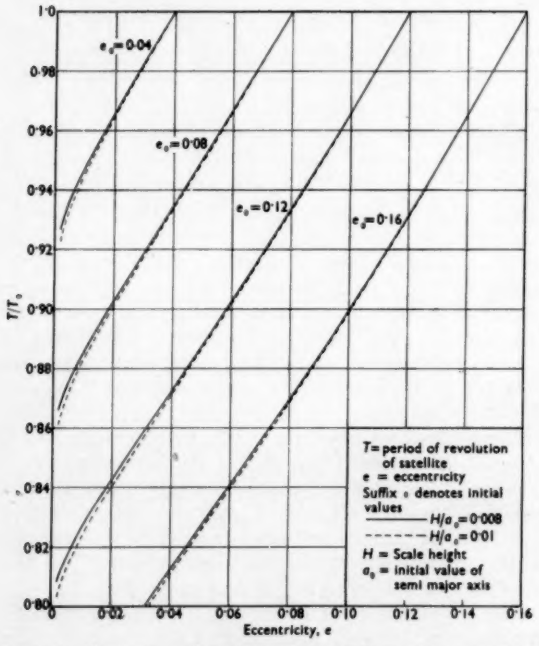

FIG. 11. Variation of period of revolution with eccentricity given by equation (23)

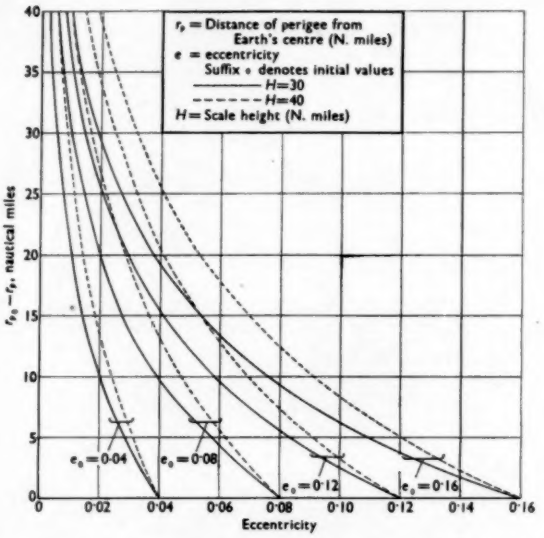

FIG. 12. Variation of perigee height with eccentricity given by equation (24)

It should be emphasized that the theoretical formulae (23) and (24) are not affected by the day-to-day variations in air density, because time does not appear in the equations.

The latitude of perigee changes slowly as a result of the Earth's oblateness, and values of the angle from apex to perigee, β, were found, assuming the initial value β_0 was known, from the theoretical formula[4] for $\dot{\beta}$:

$$\dot{\beta} = \frac{J}{2}\sqrt{\frac{g}{R}}\left(\frac{R}{\bar{r}}\right)^2\left(\frac{R}{a}\right)^{1\cdot5}\left\{5\cos^2\alpha - 1 + O(J)\right\}, \quad (26)$$

where $\frac{J}{2}\sqrt{\frac{g}{R}} = 4{\cdot}99$, if $\dot{\beta}$ is required in degrees per day.

The variation of a and $\bar{r}$ with time shows slight irregularities, reflecting the irregularities in T; so it is probably best to integrate (26) numerically. If this is done every 14 days, using the simplest form of integration,

$$\beta_{\nu+14} = \beta + 7\,(\dot{\beta}_\nu + \dot{\beta}_{\nu+14}) \quad .. \quad (27)$$

(where β_ν is the value of β at 00 hours on day ν), the error in β accruing in each 14 days as a result of the integration process should, for a satellite with inclination α between 61° and 66°, be less than 0·002°, which is much

less than the possible error due to neglect of the $O(J)$ term in (26), about 0·1° in 14 days.

The accuracy of the orbital elements, as determined in this way, depends chiefly on the accuracy of the predicted T. If the orbital elements are being predicted a fortnight ahead, a typical error in T for a short-lived satellite (with a life-time of less than one year) is 0·2 min., corresponding to an error of 5 n. miles in a and 0·0015 in e. So a typical error in height is 5 n. miles if β is accurately known, or 8 n. miles if β is in error by 1°. The error in β depends on the time which has elapsed since the last accurate observed value was obtained: the error would be about 2° per year for satellites with inclinations between 61° and 66°. For satellites at other inclinations the error would be greater.

If the process described here is used to obtain the orbital elements at a past time, much better accuracy is possible, with T correct to 0·005 min., a correct to 0·1 n. miles, e correct to 0·0005 (for $e > 0{\cdot}01$, assuming errors of 1 n. mile in r_{p0} and 5 n. miles in H), and $\beta - \beta_0$ correct to 2° per year (for inclinations between 61° and 66°).

For prediction purposes, it will often suffice to take the inclination of the orbit to the equator as constant. The inclination does change slowly however, and if, as is probable, this change is due to rotation of the atmosphere, it is a legitimate rough approximation to assume that α varies as $T \sin \alpha_0$, *i.e.*,

$$\alpha = \alpha_0 - C(T_0 - T) \sin \alpha_0 \quad .. \qquad .. \quad (28)$$

The observational results for *Sputnik 2* suggest that the value of C is near 0·01, if α is in degrees and T in minutes.

VI. CALCULATING THE TRACK OVER THE EARTH'S SURFACE

In making the calculations for the track diagram the orbit was assumed to be an exact ellipse. In fact the distance r of the satellite from the Earth's centre departs from a true ellipse by $0{\cdot}94(R/\bar{r})\sin^2\alpha$ n. miles, being smaller nearer apex and greater near the equator.[4] This perturbation can be ignored since it is much smaller than the likely error in predicted height. The time from apex is also slightly different on the perturbed ellipse, but the discrepancy never amounts to more than 0·1 sec.

If, as shown in Fig. 13, ψ is the angle from apex to the satellite, measured in the orbital plane, and $\psi = \beta$ at perigee, the distance r of the satellite from the Earth's centre is given by the standard polar equation of an ellipse,

$$r = \frac{a(1 - e^2)}{1 + e \cos(\psi - \beta)}, \quad .. \qquad .. \quad (29)$$

and the height above the Earth's surface, y, may be found by subtracting the local Earth radius $R_\lambda = R(1 - 0{\cdot}00335 \sin^2\lambda)$, where λ is the latitude.

The time from perigee, $\tau - \tau_\beta$, is given by Kepler's equation,

$$\tau - \tau_\beta = \frac{T}{2\pi}(E - e \sin E) \quad .. \qquad .. \quad (30)$$

where E is the eccentric anomaly, which is related to the true anomaly $(\psi - \beta)$ as shown in Fig. 14, and may be expressed in the form

$$\sin E = \frac{r \sin(\psi - \beta)}{a\sqrt{1 - e^2}} \quad .. \qquad .. \quad (31)$$

Taking $\tau = 0$ at apex, when $\psi = 0$, equations (30) and (31) give

$$-\tau_\beta = \frac{T}{2\pi}(E_0 - e \sin E_0) \quad .. \quad (32)$$

and

$$\sin E_0 = -\frac{\sqrt{1 - e^2}\sin\beta}{1 + e\cos\beta} \quad .. \qquad .. \quad (33)$$

Equations (29)–(33) serve to define r and τ in terms of ψ, and it only remains to relate ψ to latitude and longitude.

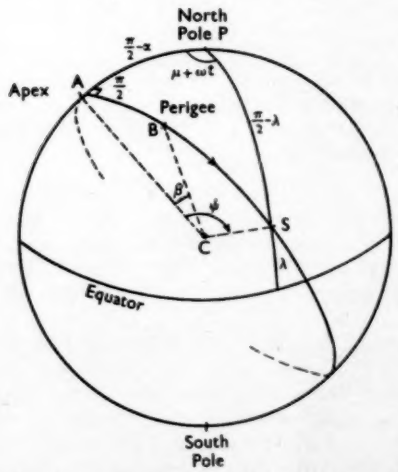

FIG. 13. Geometry of the orbit. *ABS* represents the track of the satellite over a reference sphere whose centre C is at the centre of the Earth

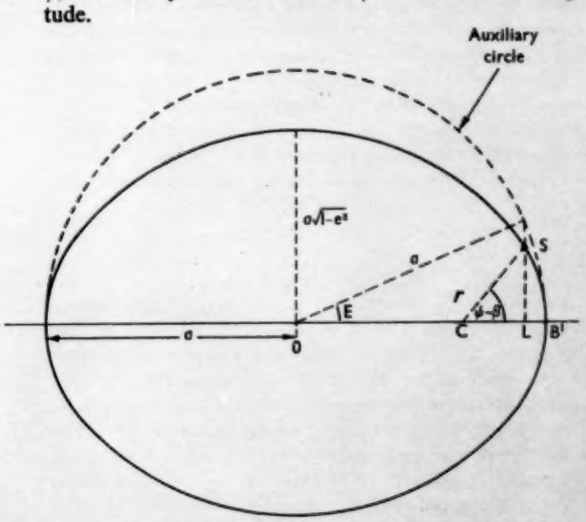

FIG. 14. Definition of eccentric anomaly E

The reference sphere drawn in Fig. 13 is assumed to rotate about the Earth's axis at the same rate as the orbital plane, so that the arc *ABS* is a great circle. If μ is the geographic longitude east of apex and ω the angular velocity of the Earth relative to the reference sphere, the angle *APS* in Fig. 13 is equal to $\mu + \omega\tau$. The value of ω is found by adding the angular velocity of the Earth and the angular velocity of the orbital plane about the Earth's axis.

$$\omega = \frac{360{\cdot}986 + \dot{\Omega}}{1440} \text{ deg./min.}, \qquad .. \quad (34)$$

where $\dot{\Omega}$ is given by (14). Finally, from the spherical triangle *ASP*, we have

$$\cos\psi = \frac{\sin\lambda}{\sin\alpha} \qquad .. \quad .. \quad .. \quad (35)$$

and

$$\mu = -\omega\tau + \sin^{-1}\left(\frac{\sin\psi}{\cos\lambda}\right) \qquad .. \quad .. \quad (36)$$

Equations (29)–(36) provided the basis for the track diagram. First, the constant τ_β was computed from (32) and (33), the values of β, e and T being known. Then a series of values of latitude λ was chosen; for each λ, ψ was obtained from (35), r from (29), E from (31), τ from (30) and μ from (36). The longitude and height at one-minute intervals were then found by plotting μ and $(r - R_\lambda)$ against τ. (R_λ, which lies between 3432 and 3437 n. miles for $40° < \lambda < 65°$, was in practice taken constant at 3435 n. miles, an approximation which could well have been abandoned).

There are some unstated simplifications in the process described above. λ, which is really the geocentric latitude, is tacitly assumed equal to the geographic latitude. These two latitudes can differ by nearly 0·2°, which is of the same order as the expected error in longitude. Ignoring this difference is therefore an assumption which is only just acceptable, and a correction, which could be made quite easily, would be required in any more accurate analysis. The second simplification is concerned with the orbital inclination α, which is not strictly constant during one revolution but varies, on a typical orbit, by up to about 0·02° between equator and apex. Obviously this change can legitimately be ignored.

VII. VISIBILITY

The first question which arises when determining whether a satellite is illuminated by sunlight is the allowance to be made for atmospheric refraction. Three possibilities were tried: single refraction, 0·6°; double refraction (once on entering the atmosphere and once on emerging), 1·2°; and no refraction at all. The last assumption fitted observation best and was adopted.

Under this assumption the satellite will be illuminated if the angle η between the local horizontal and the line from the satellite to the horizon is greater than the angle of depression ξ of the centre of the Sun below the horizon. As Fig. 15 shows, η is given by

$$\sec\eta = 1 + \frac{y}{\bar{R}} \qquad .. \quad .. \quad (37)$$

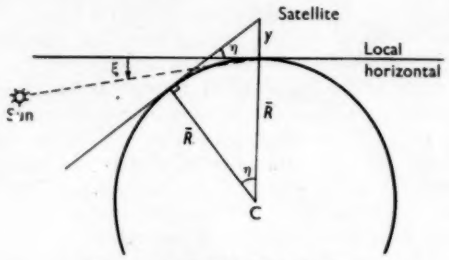

FIG. 15. Visibility of satellite

if the Earth is taken as spherical, of radius $\bar{R} = 3435$ n. miles. The angle of depression ξ of the Sun, at a given point (latitude λ north, longitude μ east) and a given time, is found by applying the cosine formula to the spherical triangle *GPZ* in Fig. 16:

$$\sin\xi = -\sin\lambda\sin\delta_s - \cos\lambda\cos\delta_s\cos h_s, \qquad .. \quad (38)$$

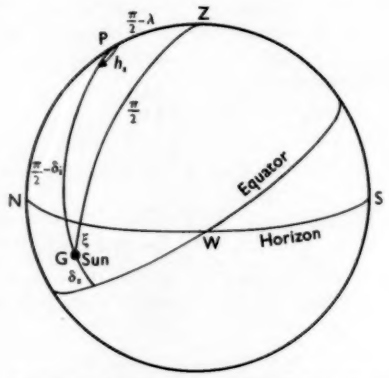

FIG. 16. Angle of depression ξ of the Sun

where δ_s is the Sun's declination, obtained from the Nautical Almanac, and h_s is the angle *ZPG*, the hour angle of the Sun. h_s is given by

$$h_s = 15\,(\text{G.M.T.} - 12 + \Delta) + \mu \text{ degrees}, \qquad (39)$$

where G.M.T. is expressed in hours and decimals of an hour, and Δ is the equation of time, which varies during the year between +0·28 and −0·24 hours. The satellite enters (or emerges from) eclipse at the latitude where $\xi = \eta$.

In practice equations (37)–(39) were used in the following way. The variation of ξ with latitude λ on a particular date, as given by (38), was plotted graphically for various values of h_s, as in Fig. 17, together with the variation of η with λ, values of y for use in (37) being obtained from the track diagram. The latitude of eclipse was then found by taking three or four values of λ near the expected eclipse latitude, choosing a particular

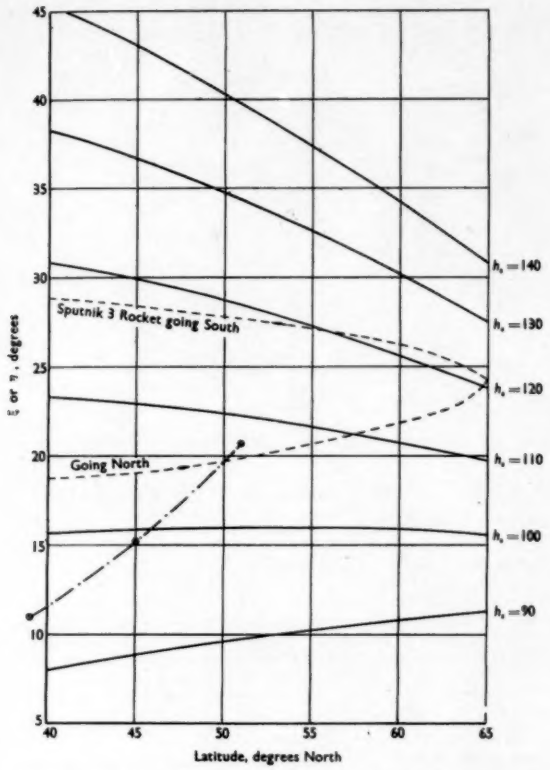

FIG. 17. Variation of ξ and η with latitude for 1958 October 27.0

h_s = hour angle of the Sun (see equation 39), degrees.
——— ξ = angle of depression of Sun
– – – – η (see Fig. 15 for definition)
○—·—○ values of ξ for transit at 18.45 G.M.T. on 26 October, 1958

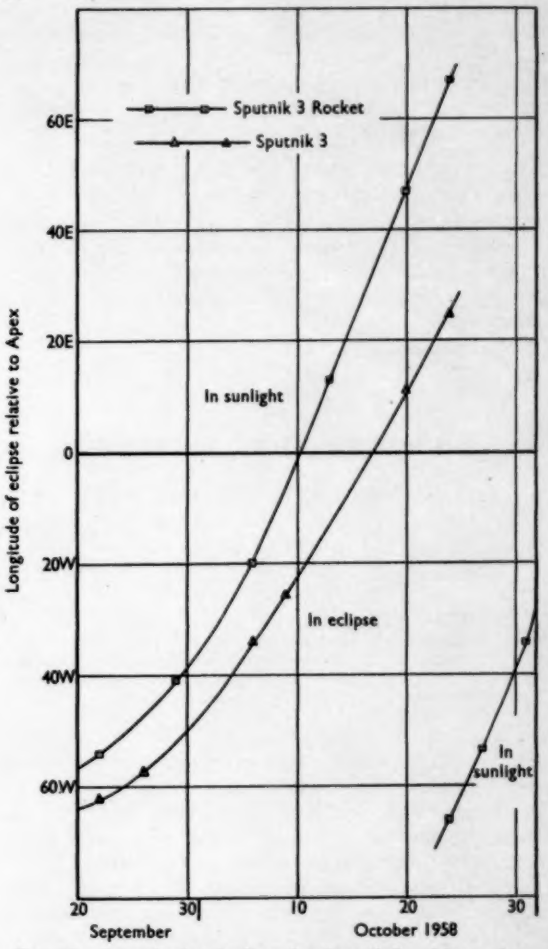

FIG. 18. Longtitude of eclipse for *Sputnik 3* and its rocket

transit on the selected date to obtain corresponding values of G.M.T. and μ, evaluating h_s from (39), and plotting the resulting values of ξ on Fig. 17, to form a curve whose intersection with the η-curve gives the latitude of eclipse. For purposes of presentation eclipse latitude was, by means of the track diagram, converted to eclipse longitude, east or west of apex, and this was then plotted against time, as in Fig. 18. Extrapolation of past values of eclipse longitude in Fig. 18 provides the value of "expected eclipse latitude" which is needed to start the calculation process.

The method described above has the virtue that the eclipse longitudes for a sheet of setting data, containing perhaps 50 transits, can be determined and written down in about ten minutes. But some calculation is required beforehand, to obtain the values of ξ and η for Fig. 17. To keep the errors in the position of the Sun and the satellite reasonably small, it was found necessary to draw graphs like Fig. 17 at intervals of one week.

Another limit on visibility is the brightness of the sky, but this will be affected a great deal by local weather conditions: if a bank of heavy cloud obscures the setting Sun, a satellite in the east is much more likely to be visible. For prediction purposes, *Sputniks 2* and *3* were assumed to be visible if the Sun was more than 6° below the horizon, the limit of civil twilight; but there were wide variations about this figure.

VIII. CONCLUSION

Using the methods described in this article, one person with a desk calculating machine can undertake prediction for two or three satellites. The methods are most suitable for the satellites which are most difficult to predict, those with life-times of less than one year. When the life-time is greater than, say, five years, much more accurate prediction is possible, and more elaborate prediction methods may be profitable.

The method of presentation in the form of a track diagram is most appropriate for satellites whose orbital inclinations lie between 55° and 75°. At other inclinations the major axis of the orbit rotates more rapidly, and track diagrams would have to be issued very frequently, perhaps even daily.

ACKNOWLEDGEMENTS

We wish to thank Mr. D. H. Sadler, Dr. J. G. Porter and Mr. G. E. Taylor of H.M. Nautical Almanac Office, Royal Greenwich Observatory, who devised the form of presentation of the predictions in terms of apex time and apex longitude, and have given us much useful advice.

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PERTURBATION OF ELLIPTIC ORBITS BY ATMOSPHERIC CONTACT

II.—SOME DEDUCTIONS FROM OBSERVATIONS OF THE ORBITS OF THE FIRST RUSSIAN SATELLITES*

By T. R. F. NONWEILER,† B.Sc., A.F.R.Ae.S., A.F.I.A.S., Fellow

ABSTRACT

This paper reviews information at present available concerning the action of air resistance on the orbit of the first Russian satellites, in the light of the theoretical treatment previously given by the author. As such it is treated as a continuation of this previous paper. It is found that the theoretical results on orbital decay are of use in determining certain features of the satellite, though for prediction purposes proper allowance for Earth oblateness is necessary. There is apparently a considerable variation from day to day in atmospheric properties within the ionosphere but mean conditions can be inferred.

THE previous article,[6] which examined the atmospheric retardation of satellites on the basis of a simple theory, was completed at about the same time as the first man-made satellites were launched, and it is naturally of interest to determine how far their behaviour corresponded with the predictions of this theory. This correspondence is far from exact, but the disparity between prediction and observation leads to some interesting speculations (if not firm conclusions).

The observational data on which the present analysis is based have kindly been supplied to the author by D. G. King-Hele of the Royal Aircraft Establishment, Farnborough, and (at the time of writing) have not all been published elsewhere. The data are of a provisional form and subject to correction, but it is not anticipated that the corrections needed would be very severe. The observations, of course, are all concerned with the geometry of the orbit and its time variation, and it is not to be expected that we can be as firm about inferences drawn from these features as if they were derived from telemetered instrument recordings. The deductions from the latter will no doubt become available in due course, though in relation to the first two Russian satellites 1957α and 1957β‡ (which are the only two whose behaviour is commented upon here) the telemetered information is understood to have been available for only a short duration after launch.

I. OBSERVATIONS OF 1957β

Of these two Russian satellites, the second when visible had its orbit carefully followed by kinetheodolites based in this country, and the observations are frequently of sufficient accuracy to yield a figure for the period of the orbit correct within much less than one part in 10^5, and to place the distance of the perigee (and so also of the apogee) from the Earth's centre correct within a mile. These two characteristics uniquely determine the shape of the orbit. Other data also determined the position of the orbit relative to the Earth, and although these are irrelevant to an analysis which assumes a spherical Earth with a spherically symmetric atmosphere, we shall later wish to employ them when examining the sources of error in the analysis.

It will be the task here to relate the changes in shape of the orbit (as shown by the changes in period and perigee distance) to the assumed form of structure of the atmosphere needed to account for them.

* Manuscript received 16 June, 1958.
‡ *I.e.*, Sputniks 1 and 2, respectively.

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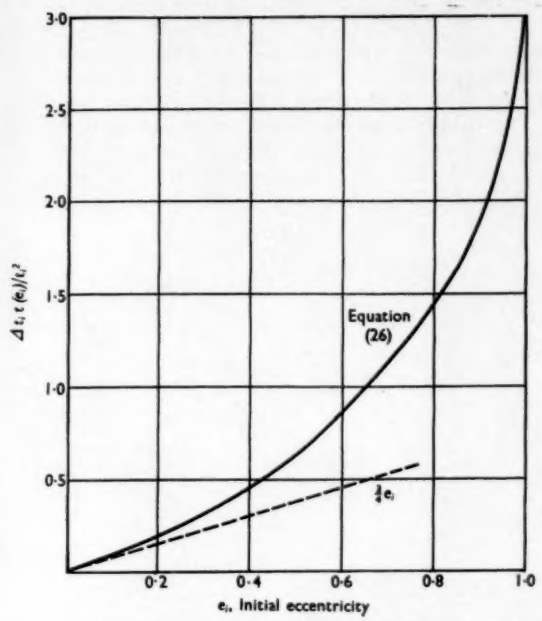

FIG. 3. Graph determining lifetime $t(e_i)$ from knowledge of period (t_i), rate of decay of period ($\Delta t_i/t_i$) and eccentricity of orbit (e_i); see ref. 6

1. Rate of Drop of Perigee

The rate of fall of the perigee was initially only about 250 ft. per day and was therefore too small to be detectable on a day-to-day basis. Indeed it is only over durations of the order of the lifetime of the satellite that any significant rate can be calculated. However, it is a useful item of information, as it is directly related to the density "scale" height H_0 (*i.e.*, the rate of change with height, close to the perigee of each orbit, of the logarithmic air density). The most convenient interpretation on the basis of the analysis of the first part can be derived from equations (15) and (24) to be

$$t(e_0)\frac{dh_0}{dt} = \frac{(1-e_0)^{3/2}}{4e_0}\left[\frac{j_0(\lambda_0/2\epsilon)}{j_1(\lambda_0/2\epsilon)} - 1\right]\times \times\left[T(e_0) - T(0)\right](R+h_0) = \tfrac{1}{4}H_0\left(1 - \frac{5}{6}e_0 + \ldots\right)\left(1 + \frac{3}{2}\frac{\epsilon}{\lambda_0} + \ldots\right) \quad (27)$$

and the orbital data fitted a value of $t(e_0)\frac{dh_0}{dt} = 8{\cdot}2$ miles, without significant change with time (at least up to the last few days of existance). Here $t(e_0)$ denotes the remaining (theoretical) lifetime of the satellite as calculated by equation (24), and h_0 the height of perigee above the (spherical) mean surface of the Earth. Due to the lack of precise observation of h_0, it seems doubtful whether initially the value of H_0 so calculated is more exact than within about ± 4 miles, but with this reservation we can estimate from (27) a value of $H_0 = 33$ miles.

This is more than double the value of H_0 associated in Table I with the perigee height involved, which latter progressively dropped from about 140 miles above the mean surface. It is now, of course, recognized that estimates of the properties of the ionosphere (such as those of Table I) erred seriously on the side of underestimating air density, and this large value of H_0—denoting a less rapid fall in density with height—is one reflection of this error. The trend has apparently been still further confirmed at much greater altitudes by the data from the first American satellites.

2. Estimates of Air Density in the Ionosphere

The difficulty of deriving the air density from the other information—namely, the rate of change of orbital period—is due to the fact that the density only enters through the term representing drag, and the shape of the satellite 1957β is not sufficiently well-defined for any great confidence to be placed in an estimate of its drag. But without any drag estimate it is possible to deduce the variation of air density at perigee relative to (say) that at the initial perigee, provided we are willing to accept the simplifications of the theory already given. Then, from equations (4) and (20) in (13):

$$\frac{\Delta t_0/t_0}{\Delta t_i/t_i} = \frac{a_0(1+e_0)j_0(\lambda_0/2\epsilon)}{a_i(1+e_i)j_0(\lambda_i/2\epsilon)}\left(\frac{\rho_0}{\rho_i}\right)$$

and the variation of (ρ_0/ρ_i) on this basis is plotted against height of perigee above the mean surface in Fig. 4. We shall have more to say later about the apparent scatter of the data, but it is evident that they are not reconcilable with the value of H_0 derived before, as they indicate a density scale height of only about 20 miles. In other words, if we are to believe the data, the drag apparently increased more rapidly than would be accounted for by

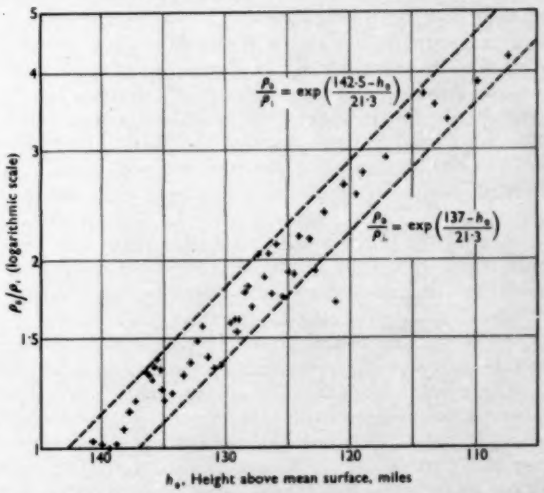

FIG. 4. Variations of (ρ_0/ρ_i) with height of perigee above mean surface

the measured rate of increase of density with height—the disparity being too large to be accounted for by any error of the latter. Alternatively, the perigee dropped more rapidly than was occasioned by the rate of decrease of density away from the perigee.

Such a disparity must be due to the breakdown of one of the assumptions of the simple theory, and it seems most probable that it may be an effect of the Earth's oblateness, as we shall now attempt to explain. The possibilities of errors from this source have always been in mind, and have received some brief attention in the columns of *Nature*.[7,8]

3. Effect of the Earth's Oblateness

The effect of the Earth's oblateness on the orbit *in vacuo* has been the subject of a recent and searching treatment by King-Hele.[9] It manifests itself in three ways:

(*i*) the orbital plane precesses about the Earth's axis;

(*ii*) the distance of the satellite from the Earth's centre (r) does not follow a precisely elliptic variation but bulges in the plane of the equator; and

(*iii*) the major axis of the orbit rotates in the orbital plane.

In trying to assess the effect on atmospheric perturbations it is necessary to make some new assumption about air density variation, and it would perhaps seem plausible to take the density as being identical all over the Earth at equal heights above the local surface, and unchanging with time.

A full treatment of all these modifications would be very involved, but it appears that the orbital precession by itself has no effect on the perturbations. The changed form of the law governing density variation with distance from the Earth's centre, and the changed shape of the satellite's path, both have some effect on the perturbation from each circuit; but this is found to depend on the ratio of such quantities as the flattening of the Earth (some 13·3 miles), and the amplitude of the non-elliptic variation of r (of the order of 2 miles), to the difference between major and minor axes of the basic orbit. As the latter was initially some 800 miles for satellite 1957β, the effect on its perturbations is relatively small, except right at the end of its lifetime.

However, the third-named effect—the rotation of the major axis—does have an appreciable effect in that it causes the air density at closest approach to increase as the major axis approaches the equatorial bulge, on top of any other increase arising from the contraction of the orbit by atmospheric perturbations. The quantitative effect here depends on the ratio of the rates of decrease of perigee instantaneous altitude derived from the two causes (defining "instantaneous altitude" as the distance from the Earth's centre, less the local Earth radius); this can be shown to be of the order of the ratio of Earth flattening to density scale height.

4. Effect of Rotation of the Major Axis

An analysis of this effect is given in the appendix, the assumptions about the variation of Earth radius and air density being shown there in equations (28) and (29).

The data quoted earlier in regard to atmospheric density variation (deduced from the 1957β orbit) are re-plotted in Fig. 5, using equation (30) of the appendix

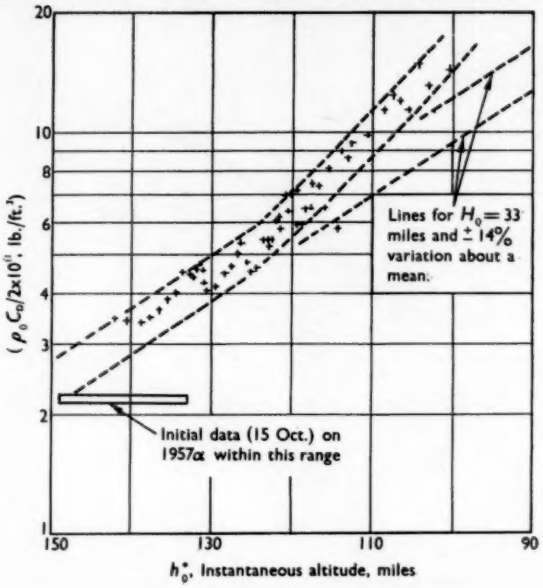

Fig. 5

for the instantaneous altitude at closest approach. A rotation of 0·4 deg./day of major axis was used to obtain this figure, as calculated from equation (31) of the appendix and indeed observed to be approximately valid; accordingly, the perigee of the orbit reaches the equator towards the end of the satellite's lifetime. The density is shown in dimensional terms by defining the reference area for C_D so that $m/S = 60$ lb./ft.^{2}; this particular loading was chosen to correspond to four times the quoted "instrumentation" mass of 508 kg. on a circular base of 1½ m. radius. Whether, for instance, the drag is properly estimated then by taking $C_D = 2$ on this area, or whether indeed these are appropriate figures for mass or frontal area, must remain in doubt.

As will be seen, the data of Fig. 5 are compatible with a density scale height of 33 miles (in accord with the estimate from (27)) at least for perigee instantaneous altitudes above 125 miles (*i.e.*, for the first 16 weeks of the 23-week lifetime). Below this altitude there is a significant tendency for the scale height to decrease, but of course it is very reasonable that it should do so, as it is known to be only about 3 miles at an altitude of 60 miles.

The deduction of equation (27) remains unaltered by such considerations as those we have made of the effects of the Earth's oblateness, provided it is remembered that $t(e)$ is to be interpreted in this formula as the

theoretical lifetime, given by the (unmodified) equation (24). The basic law expressed by (27) is that of equation (15) which has not been altered by our corrections—as h_0 is the height above a mean Earth surface.

5. Lifetime Estimates

Thus it seems that the derived correction of oblateness accounts for the incompatability between the values of scale height derived independently from assessments of the rate of decrease of perigee height and period. The prediction value of the new forms $t^*(e)$ and $N^*(e)$ of equations (21*) and (24*), also derived in the Appendix, is certainly much greater than that of the simpler forms given in the corresponding equations (21) and (24). To illustrate this, equation (27) gives the 1957β lifetime as 162 days on the basis of the observed orbit and the value of $\Delta t_0/t_0$ made on 1 December; the use of the modified form $t^*(e)$ of (24*) gives only 141 days—the reduction being accounted for, of course, by the increase in density as the perigee swings round to the equator. The actual lifetime from the state of the observation was still shorter (133 days). But the improvement effected by the oblate Earth correction gets rid of three-quarters of the error of the simple formula.

6. The Fluctuations in Apparent Drag

The same improvement in prediction can be noted throughout the lifetime of the satellite, though considerable fluctuations of the predicted date of demise would occur due to the oscillating character of the observed values of $\Delta t_0/t_0$—proportional, as we have noted earlier, to the drag. A prediction on current information one day might be correct, but a similar prediction on current information a few days later will be incompatable with it and wrong. These fluctuations account for the scatter of the data (within limits of about $\pm 14\%$) shown in Fig. 4 and 5, and are better seen in Fig. 6 to be some irregular oscillations. Naturally in theory the time variations—whether or not corrected for oblateness—are regular and monotonic. However, provided the oblateness correction is introduced, the theoretical curve follows closely the general trend of the observations.

The explanation of these oscillations is not at all clear. Many people have suggested that the satellite is rotating and so the drag is continually changing. Whilst it cannot be disputed that the satellite rotates, it would seem likely that it spins so fast as to obliterate any slow oscillation, such as that which would be needed to account for the observational data. Indeed, information published since this account was prepared suggests that similar fluctuations have been observed in the behaviour of the *spherical* Vanguard satellite, and of course these could not be explained as due to changes in the attitude relative to the direction of motion.

Many other interesting explanations have been suggested, but it must not be overlooked that the oscillations may simply reflect variations in atmospheric state in the

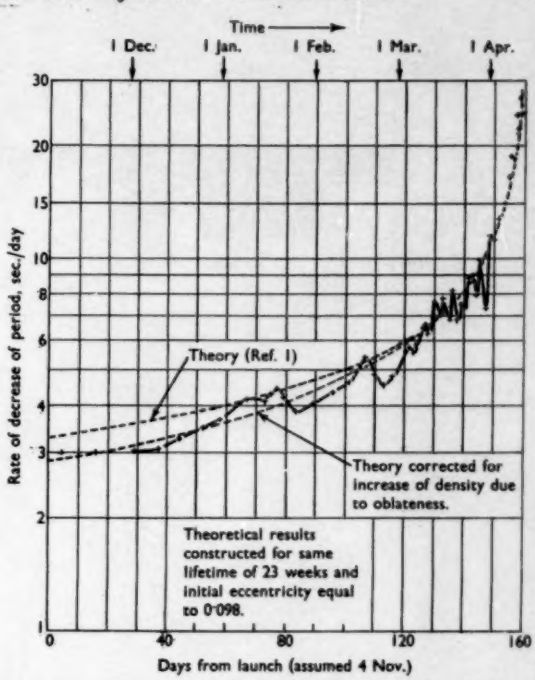

Fig. 6

ionosphere. Certainly large changes are already known to take place between day and night, though, of course, the satellite perigee moves only very slowly relative to the twilight zone (due to the combined effects of precession of the satellite about the Earth's axis, and the rotation of the Earth about the Sun), so that consecutive contacts with the atmosphere all occur at roughly the same time of day or night.

7. Solar Flares

In speculative support of this, Fig. 7 demonstrates a possible correlation between the fluctuations of the rate of period decrease and solar flare intensity. The upper block diagram denotes the daily sum of (area $\times$ duration) for a sample of solar flares (above intensity 1), averaged for convenience over time intervals between successive observations of the period of 1957β. The lower curve shows the difference between the observed rate of decrease in period for the satellite and the instantaneous theoretical mean rate. The similarity of the two curves is certainly remarkable.

If such a correlation existed, it would suggest that the flares cause a density and/or temperature decrease at the altitude of the perigee, which would be consistent with an increase in these same properties at greater altitudes.

II. OBSERVATIONS OF 1957α

The information on the behaviour of the first Russian satellite is unfortunately confined to the three weeks

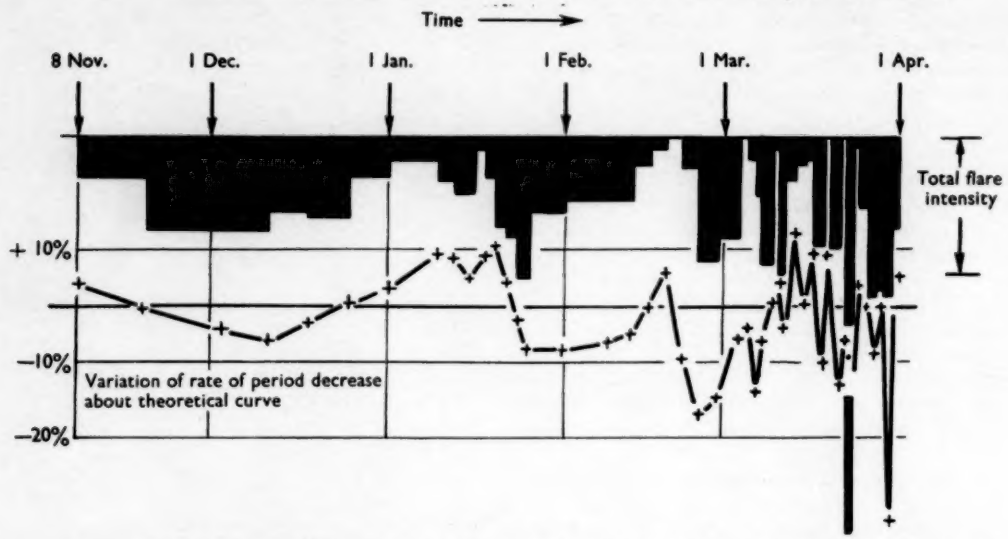

FIG. 7. Correlation between fluctuations of the rate of period decrease for 1957β and the solar flare density. The arrow with the label "total flare intensity" denotes the ordinate length corresponding to a solar flare of intensity 2 for one hour per half-day

during which its transmitter was in operation, and a knowledge of its total lifetime.

On 15 October, the orbital details[10] of relevance were observed as:

Semi-major axis (a_0)	4308 miles
Period (t_0)	5748·5 sec.
rate of decrease ($-\Delta t_0/t_0$) ..	2·28 sec./day
second rate of decrease ($-\Delta^2 t_0/t_0$)	2·35 × 10^{-3} sec./day per circuit
Eccentricity (e_0)	0·048 ± 0·002
Perigee latitude (Θ_0)	40·9° N.
at instantaneous altitude (h_0*) ..	141 ± 8 miles
Inclination of orbit to equator (α)..	64° 40′
Rate of rotation of major axis ($d\beta/dt$)	−0·33°/day (theoretical)

The observed rate of decrease of period can be shown from (13) to correspond to a density of 2·2 × 10^{-11} lb./ft.^{3}, on the supposition that $\epsilon = 0{\cdot}008$ and taking the value of m/C_DS as 32 lb./ft.^{2}. This drag loading is calculated on the basis of the Russian statement that the satellite was a sphere—for which we may confidently predict that $C_D = 2$ on its frontal area—of 58 cm. diameter and 84 kg. mass. But unfortunately the precise determination of air density thus apparently permitted is of little value as the perigee height is not accurately known. Russian sources have, however, since quoted the height as 140 miles, so that the accuracy is better than claimed in Ref. 10.

The observation[11] of the second time rate of decrease over the three weeks' observations is of some interest, as it is possible to show from equations (12) to (17) that

$$-\frac{t_0\Delta^2 t_0}{(\Delta t_0)^2} = \frac{2}{3e_0}\left(\frac{1-e_0}{1+e_0}\right)\left[1 + O\left(\frac{\epsilon}{\lambda_0}\right)\right] - 1$$

This suggests an initial value of 12 for this quantity, whereas the observed value was about 40. In other words, $\Delta t_0/t_0$ was increasing three times more rapidly than it should according to the simple theory. Perhaps such a large disparity suggests fluctuations of $\Delta t_0/t_0$ similar to those recorded on the satellite 1957β, but the accuracy of the observed value is open to considerable doubt.

The lifetime on the basis of the observations made on 15 Oct. and equation (26) was 104 ± 4 days, again an over-estimate, as it was really only 81 days. Use of the oblateness correction embodied in the modified expression for $t^*(e)$ in equation (24) of the Appendix would only bring the prediction down to 95 ± 4 days. However, if fluctuations of $\Delta t_0/t_0$ really existed, then of course (as noted before) use of this rate of change to establish a lifetime would be bound to be occasionally inaccurate. Indeed, the observed increase of $\Delta t_0/t_0$ over the 19 days of observation would have provided successive estimates of lifetime which contrasted by about 37 days. The suggestion that such fluctuations exist even for a *spherical* satellite would naturally preclude some explanations of their cause.

III. OBSERVATIONS ON 1957α(1)

The rocket carrier of the sphere was also in orbit and was a visual object. However, there is apparently virtually no systematic published information on its orbit. Presumably it was roughly the same as that of

the sphere initially, and estimates[12] of its rate of decrease of period over the period about 19 October give a figure of 4·13 sec./day. This would indicate a value of (m/C_DS), and a life-time, both 57% of those of the satellite. Its actual lifetime was 42 days on 19 October, which is indeed 45% shorter than the remaining life of 1957α on that date.

IV. BRIGHTNESS MAGNITUDE OBSERVATIONS

A certain amount of doubt has been cast on the interpretation of the data leading to absolute—as distinct from relative—measures of the air density, first by statements by Russian scientific spokesmen that the density of the atmosphere at perigee was very much *smaller* than had been assumed before—though what this was is not clear—and secondly, and less contravertibly, by measurements of the brightness of the various objects in the sky.

In particular the magnitude of 1957α(1) showed[13] that it was an object which, even if it was a perfect reflector, must have had an equivalent diameter of over 30 ft., and 1957β yielded similar figures for brightness but this object was certainly observed to fade and brighten more appreciably in various aspects, as if it had a polyhedral surface; there was evidently more to it than the conical shape suggested for the instrumented portion whose brightness magnitude could not under the most favoured circumstances have reached zero. The small satellite 1957α itself was a faint optical object, quite compatible with the deduction from its stated size that its brightness could not have been more than magnitude 3 even in the zenith at perigee.

Without speculating for the moment what may be implied by the large apparent size of 1957α(1) and 1957β, but on the hypothesis, for drag estimation purposes, that they were rigid spheres of the deduced dimensions, we would deduce that, as (m/C_DS) for 1957α(1) was about 57% of that of the 58-cm.-diameter satellite, then its mass was at least 140 times that of the latter. Belief in the Russian quotation of the mass of 84 kg. for the satellite, leads therefore to the deduction that the rocket carrier in orbit weighed 12 tons! A similar comparison relative to 1957β produces a similar figure. Here then we have a problem of which the only solutions would seem to lie in imputing to the Russian scientists either an unbelievable modesty or else an unnecessary exaggeration. For it would seem that either 1957α(1) and 1957β must have been extraordinarily heavy, or else 1957α itself was much lighter than the published figure; of course, the flaw in our argument which leads to this deduction may lie in supposing that the reflecting area produced drag *pro rata*.

Indeed, there is certain photographic evidence that 1957β was shaped as a slender projectile—with a fineness ratio of between 5:1 and 10:1 according to some interpretations. Its length is put at about 70 ft., which would be reconcilable with a side area equivalent to that of 25- and 35-ft. diameter spheres for fineness ratios of 10 and 5 respectively. In this respect it agrees with the visual magnitude observations.

V. SOME SPECULATIONS ABOUT THE ATMOSPHERE

Supposing that the shapes of 1957α(1) and β were identical, and so were the masses except for the addition of the quoted "instrumentation" load to the latter, we would determine on the basis of the figures quoted in the previous section that (m/C_DS) for 1957β was between 22 and 26 lb./ft.²—corresponding to fineness ratios of 5 and 10 respectively. In other words, on the basis of the reference value of (m/S) of 60 lb./ft.² already taken, we must place C_D between 2·3 and 2·7. Such an assumption would bring the density deduced from Fig. 5 for both satellites into close agreement, as is shown in Fig. 7.

The value of $H_0 = 33$ miles at altitudes above 125 miles is, of course, unaffected by such dubious arguments, and is surprisingly high. Supposing that the temperature is not decreasing with height, then its temperature (allowing for the dissociation of molecular oxygen) must be at least 1500° K. at these altitudes, which is considerably beyond the best current estimates; it may seem strange that such a high value has not been otherwise discovered.

Again, comparison of the density data from the satellites with prior estimates (such as that of the Rocket Panel) is interesting, as the latter cannot be vastly wrong

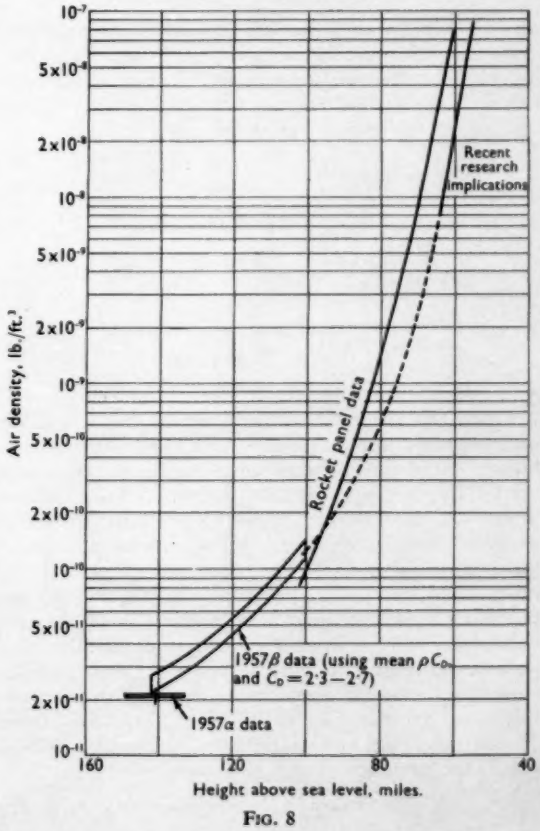

FIG. 8

at altitudes of about 60 miles. Whatever the density may be at the satellite altitudes, the graph of the logarithmic density versus height (Fig. 8) must evidently curve very abruptly between 60 and 100 miles altitude, and this curvature is a measure of local temperature gradient. In fact, the most recent information on the atmosphere at the 60-mile level suggests rather lower temperatures than those of the Rocket Panel data, and therefore rather lower densities in that region; this would be compatible with a more gradual change in slope of the graph, but the rate of change of density scale height must still imply a temperature gradient much in excess of 20° C./mile. This in turn may indicate a much more active thermodynamic reaction to be responsible for the temperature rise than had been imagined.

APPENDIX

Analysis of Effect of Rotation of Major Axis

The perturbations in each circuit are unaffected by the slow rotation of the orbit in its plane, but in summing over a large number of orbits it will appear that the air density is magnified everywhere by a factor consistent with the decrease in instantaneous altitude at closest approach. We shall suppose the Earth's radius is given by

$$R^* = R(1 + f\cos 2\Theta)$$

where Θ is the geographical latitude (zero at the equator); if the orbital plane is inclined at an angle α to the equator, and the true anomaly of the apex of the orbit (where it reaches the heighest latitude) is $-\beta$, then the latitude of the satellite is given by

$$\cos(\theta + \beta) = \frac{\sin\Theta}{\sin\alpha}$$

so that the local Earth radius below the satellite is

$$R^* = R\{1 + f[\cos^2\alpha - (\sin^2\alpha)\cos 2(\theta + \beta)]\} \qquad (28)$$

Further, the density is given by

$$\rho \propto \exp\left(-\frac{h^*}{H}\right) = \exp\left(-\frac{r - R^*}{H}\right) \quad .. \quad (29)$$

where h^* will be called the instantaneous altitude. At and near the perigee, r can be expressed in terms of the instantaneous orbital major axis and eccentricity. Strictly these are not quite equal to the instantaneous orbital characteristics at the observing station, as the orbit is no longer elliptic—but is, like the Earth distended where it crosses the equator. The error involved here, however, is about double that involved in any case by observational errors, and so can very well be ignored, particularly as the correction needed alters between northward and southward crossings of the observing station. However, it must be borne in mind that the neglect of it tends to overestimate the effect we are looking for, as the flattening of the Earth is to some extent compensated by the equatorial bulge of the orbit, though indeed the latter is a much smaller one.

Ignoring second order terms in the Earth's ellipticity, we calculate from (20) that the instantaneous altitude at closest approach is

$$h_0^* = a_0(1 - e_0) - R\{1 + f(\cos^2\alpha - \sin^2\alpha\cos 2\beta)\} \qquad (30)$$

The value of β changes with time according to the law[9]

$$\Delta\beta = t_0\frac{d\beta}{dt} = \frac{\pi JR^2}{a_0^2}(5\cos^2\alpha - 1)\,[1 + O(e)] \quad .. \quad (31)$$

where J is a coefficient, describing the variation of the Earth's gravitational potential with latitude, which can be taken as equal to $1{\cdot}643 \times 10^{-3}$ (and, of course, vanishes for a spherical Earth).

Because the density depends on the instantaneous altitude, equations (18) and (19) combine now with (29) and (30), to give, for an oblate Earth,

$$\ln\left(\frac{\rho_0}{\rho_i}\right) = \frac{h_i^* - h_0^*}{H_0}$$

$$= \frac{h_i - h_0}{H_0} - \frac{Rf}{H_0}\sin^2\alpha(\cos 2\beta_0 - \cos 2\beta_i)$$

$$= \ln\left[\frac{\lambda_i j_1(\lambda_i/2\epsilon)}{\lambda_0 j_1(\lambda_0/2\epsilon)}\right] - \frac{Rf}{H_0}\sin^2\alpha\,(\cos 2\beta_0 - \cos 2\beta_i) \qquad (32)$$

The inclusion of this extra time-varying term modifies the expressions for both $N(e)$ and $t(e)$ in equations (21) and (24). Indeed, their derivation is now no longer so easy. Within the accuracy of the expression (31) we may treat the rate of change of β as either constant with the number of revolutions, or with time. If it is further permissible to treat the density increase involved by the additional oblateness term in (32) as small—and indeed in the present example the increase (or decrease) could never be much more than 30%—then integration of the expressions for t_{i0} and N_{i0} can be carried through to yield modified expressions for $N(e)$ and $t(e)$ which we denote by an asterisk, and are as follows:

$$N^*(e) = \left(1 - \frac{Rf}{H_0}\sin^2\alpha\cos 2\beta_i\right)\left[N(e) - \frac{Rf\sin^2\alpha}{2H_0\Delta\beta}\sin 2\beta\right] \quad .. \quad (21^*)$$

$$t^*(e) = \left(1 - \frac{Rf}{H_0}\sin^2\alpha\cos 2\beta_i\right)\left[t(e) - \frac{Rf\sin^2\alpha}{2H_0}\frac{dt}{d\beta}\sin 2\beta\right] \quad .. \quad (24^*)$$

with corresponding adjustments, therefore, to the values of N_{i0} and t_{i0}. For prediction purposes, the unmodified value of t_{i0} would need to be used with (31) to calculate the value of $(\beta - \beta_i)$ corresponding to a given eccentricity.

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METEORITIC DUST AND GROUND SIMULATION OF IMPACT ON SPACE VEHICLES*

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(Communication from the Convair-Astronautics Division of General Dynamics Corporation)

ABSTRACT

A general discussion of meteoritic dust is presented and deductions pertaining to the physical properties and probable speeds are given. It is believed that a large portion of atmospheric dust is generated in the atmosphere by the disintegration and ablation of meteors. Thus, the siliceous (glass-like) and magnetic (iron-nickel) spherules, which are found on the ground, are believed to originate at meteor altitudes from vaporized meteorites. A large quantity of smaller sized dust is believed to be cometary, either arriving directly from comets, etc., *per se*, or indirectly from exploding or disintegrating meteoroids. The possibility that a portion of the influx of meteoroids may contain a sizeable percentage of frozen molecular fragments has influenced the results. For example, from this hypothesis, the possibility of a dust cloud surrounding the Earth and extending out for several thousand miles has been suggested. Also, the correlation between prominent meteor showers and excessive rainfall, snow cover and cirrus cloud, as noted by Bowen,[1,2,3] can be explained on this basis.

Finally, a proposal for accelerating dust particles to speeds of 10 miles per second, and possibly up to 18 miles per second, *in vacuo*, is presented.

I. INTRODUCTION

MATERIAL bodies from our solar system are continually entering the Earth's atmosphere with speeds ranging from 7 to 45 miles per second. They range in size from cometary nuclei, which might arrive every 50 to 80 million years (and, incidentally, we are about due for another collision), down through crater-producing meteorites, meteorite falls, detonating bolides, fireballs, visual and photographic meteors, telescopic and radio meteors, and, finally, a class of debris which is too small to produce visual or radio meteors, meteoritic dust. We know that the meteoritic dust exists by deduction. Also, there is meteorological and geophysical evidence for an influx of extremely fine dust. The zodiacal cloud is believed to consist of dust particles and the solar corona can be attributed to an influx of dust, in part. The Gegenschein can probably be explained on the basis of a dust cloud which may surround the Earth and extend out several thousand miles. In addition to the observational evidence for interstellar dust, it is known that dust particles are present in comet tails.

A variety of measurements can be made photographically during the descent of brighter bodies—such as luminous intensity, speed, deceleration, etc.—and from these data a plot of probable mass versus number can be made. It is found that the smaller sizes tend to increase logarithmically. An extrapolation of these data would indicate that the very tiny particles are much more numerous. However, this is only a guess and the range of speeds is not known, although it is often assumed to lie between 7 and 45 miles per second. If, indeed, the meteoritic dust particles are most numerous, then it is these particles that space vehicles are most likely to encounter, and consequently, they are of considerable interest at the present time. As previously mentioned, meteoritic dust is too small to produce visual meteors, and therefore it is necessary to resort to deduction in determining the physical properties and speeds of these particles. This is what we shall attempt to do, and after we have established a rough idea of what meteoritic dust consists of, *i.e.*, a rough idea of what we are trying to simulate, including speeds, a technique will be described which it is believed will enable dust particles to be rapidly accelerated to speeds of over 10 miles per second. One of the chief advantages of a ground facility for producing high-speed dust is that it would enable the calibration of rocket-borne dust-collecting instruments.

For a working definition, the term meteoritic dust will be used to designate small particles, which have arrived in the Earth's atmosphere either directly in the form of dust, *per se*, or indirectly from larger meteoric bodies which have disintegrated or ablated. The latter source might be important, for all of the incoming debris, except that which lands intact (about six a day throughout the world of the visual meteors), must eventually turn into smoke, dust or gas.

Now, in general, it can be said that almost all, if not all, of the incoming debris can be traced to the asteroids and the comets, or at least to bodies with asteroidal or cometary orbits. From observations made with the Baker Super-Schmidt meteor cameras, Whipple[4] states that at least 90 per cent. of the photographic meteors have cometary orbits, while those with asteroidal orbits constitute less than 10 per cent. Interstellar meteors (meteors with hyperbolic orbits) do not exceed more than 1 per cent. of the entire influx if they exist at all. The cometary and asteroidal bodies appear to be distinct species with few exceptions, and, accordingly, they have been given distinguishing names. Asteroidal bodies are called meteorites while cometary bodies are called meteoroids. However, since the asteroids and comets

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can hardly be expected to have unique physical properties, the preceding sentence cannot be considered a definition. That is, bodies resembling meteorites or meteoroids may also come from other sources. The terms meteorite and meteoroid will be defined in the following sections. It should be pointed out, however, that we are using the word meteorite to cover a much larger class of objects than ordinarily implied. By meteorite one usually refers to the remains of a meteor that has hit the ground. Nevertheless, there are connotations; for example, one talks about spaceships colliding with meteorites, etc. Since the physical properties of meteorites are not appreciably changed in passing through the atmosphere, except for the surface ablation, the connotations appear to be justified.

II. THE ASTEROIDAL INFLUX: METEORITES

The meteorites are hard, solid, high-density materials which occasionally hit the ground. About 1,563 of them have been recorded and studied.[5] In general, they consist of stones (aerolites), or metals (siderites), the latter mostly iron and nickel, or mixtures of both (siderolites). About 20 per cent. of the meteorites are of the familiar iron-nickel composition, or about 2 per cent. of all incoming debris is of this variety. It is believed that the meteorites or asteroidal particles were originally a part of one or more larger bodies which existed between Mars and Jupiter and somehow managed to break apart, possibly by collision. At the present time there is evidence to indicate that the majority of asteroidal bodies are larger than a few millimeters in diameter. There are several reasons for this, *e.g.*, the radiation pressure of the Sun will tend to remove particles with diameters of the order of a few microns or less, depending on several factors such as density, shape, etc. Larger particles will tend to spiral into the Sun as prescribed by the Poynting-Robertson effect. This is a relativistic phenomenon involving the Sun's radiant heating and the subsequent reradiation which causes a back thrust, tending to oppose the bodies' motion. The effect is small but over long time periods it can be very significant. For the sake of calculation, Wyatt and Whipple[6] found that in a period of 3×10^9 years, all particles less than 8 cm. in diameter would have been swept into the Sun from the asteroids by this phenomenon. In 10^7 years all particles with diameters less than 0·08 cm. would have been wiped out. Furthermore, Whipple has recently pointed out that the heating from low energy solar protons would enhance this effect. However, a certain amount of dust will be continually generated within the asteroid belt by collisions. Piotrowski[7] believes that dust generated in this fashion may be supplying the zodiacal cloud. A lot of it would undoubtedly be trapped by the great planet Jupiter. The amount supplied to the Earth is unknown.

1. SPHERULES

At meteor altitudes the large majority of meteorites will vaporize and condense into small droplets which are commonly called spherules. As one would expect, there are two common types of spherules: the first is the siliceous spherule which comes from stony meteorites and has a glass-like appearance; while the second is the black magnetic spherule which comes from the iron-nickel meteorites.

The magnetic spherules are black because of an oxide crust which resembles magnetite. Many are unoxidized in the interior and have a metallic appearance when the crust is broken off, as illustrated in Fig. 1.

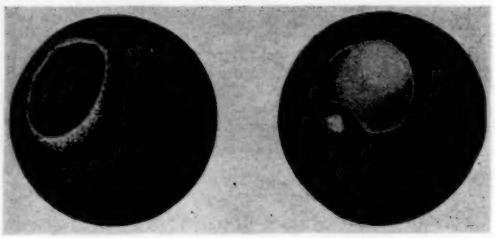


FIG. 1. The black magnetic spherule to the left has an indentation (cupule) which is found on many spherules. The other spherule has the surface chipped away to show the shiny metallic interior. Both spherules were reproduced from an old photograph by J. Murray[8]

Because of the manner in which they are formed, the spherules have an upper and lower limit to their diameters. The upper limit comes from the stability of a liquid droplet while the lower limit stems from the fact that work must be done against surface tension each time a droplet is subdivided. As the droplet becomes smaller, the energy density, needed to break the droplet further, becomes increasingly larger, since the surface-to-volume ratio increases as $3/r$, where r is the radius. Öpik[9] calculated that $10\,\mu$ was a typical lower limit to the diameters. He also found that $250\,\mu$ was about the upper limit. The observations of Laevastu and Mellis[10] of black magnetic spherules from a long sediment core in the equatorial Pacific Ocean are in accord with Öpik's theory. They separated 880 undamaged specimens and found an average diameter of 44 μ, the entire lot ranging in size from 10 to 230 μ. The mean specific gravity was 5·2. Buddhue[11] collected 1,795 magnetic and 454 siliceous spherules and found average diameters of 20 and 27 μ for each type, respectively. The mean specific gravity of the siliceous spherules was 2·8. He also found that the spherules had a size distribution that satisfied the compound interest law, *viz.*, if N is the number of spheres having diameter D, then

$$N = ae^{-bD},$$

where a and b are constants. Laevastu and Mellis state that the product ND^3 is approximately constant, the diameter being measured in 10 μ intervals. Raindrops are distributed in a similar fashion.

On the basis of the work of Laevastu and Mellis, about 125 tons of black magnetic spherules reach the ground each year. If the siliceous spherules outnumber

the magnetic spherules by the same ratio as stony to iron meteorites, then about 500 tons of the latter also reach the ground each year. We therefore might expect about 625 tons of spherules to be formed in the atmosphere at meteor altitudes each year, on in round numbers, about 1,000 tons per year.

2. The Influx of Nickel

In 1952 Pettersson and Rotschi[12] concluded that the nickel content of their collected deep sea deposits originated in part from meteoritic dust. Assuming the same rate of deposition for the entire world, they arrived at a value of a million tons per year! This result was widely quoted and it caused quite a few re-estimates in the amount of incoming debris. However, in 1954, Wiseman and Smales,[13] after studying the significance of the nickel and cobalt content of deep sea deposits, concluded that the igneous rocks are the ultimate source of nickel in deep sea sediments. From the additional work of Ishibashi[14] and Goldberg[15] it can be ascertained that the majority of free nickel in deep sea deposits is brought about chemically by co-precipitation with iron and manganese.

III. COMETARY INFLUX

We are therefore left with cometary matter (comets, meteor streams, debris with cometary orbits) as the principal remaining source of meteoritic dust. If it is assumed that the majority of such debris is related to comets, possibly the remains of ancient comets or fragments of comets, then a knowledge of the comet, *per se*, should lead to a better understanding of all such materials.

Very little is known regarding the origin of comets. Those which are new to the inner solar system (long period comets) arrive from random directions in space. From the associated orbital studies it is evident that there must be a cloud of comets at great distances surrounding the Sun. Oort[16] has shown that this cloud must extend out to at least 40,000 astronomical units and that it might contain as many as 10^{11} observable bodies. Öpik[9] demonstrated many years ago that the Sun was indeed capable of maintaining such a cloud. In fact, even at a distance of four light years, it would be possible for the Sun to maintain a cloud of comets for a period of three thousand million years without the loss of the majority of them by intruding stars. Passing stars would only disrupt a few because of the high speeds involved. Öpik has compared this with the passage of a bullet through a swarm of gnats.

However, Oort believes that the continual re-shuffling of the velocity distributions in the cometary cloud by star perturbations may be what motivates new comets towards our Sun.

Ordinarily comets are not seen until they come within one or two astronomical units of the Sun. At that time the star-like nucleus may develop a sizable coma which consists of gas and dust. The coma, being much larger than the nucleus but not as bright, may appear as a hazy patch of light which is too faint to permit spectroscopic examination.

As the comet continues towards the Sun the brightness may increase faster than the inverse square law predicts and there may be explosive increases in light corresponding to two or three orders of magnitude. However, in general the intensity is not symmetrically distributed along the orbit with respect to the major axis. In fact it is usually greater following perihelia.

The tail is the third recognizable feature of comets. It may be nothing more than a part of the coma which is blown away from the Sun by various forces. However, it is generally recognized that there are three distinct types of tails: the first consists of ionized gases, which produce streamers and envelopes; the second consists of dust particles; and the third is mostly non-ionized gases. The motions of the last two types can be explained on the basis of the Sun's radiation pressure and gravitation alone. The high accelerations of the first type are subtle and more difficult to explain. However they are strongly influenced by beams of particles which stream out from the Sun with a locked-in magnetic field. Interactions between the ions and the fast moving magnetic field may be what accelerates them to speeds of 70 km. sec.$^{-1}$

Ordinarily stars are not occulted by any part of a comet. They have been reported to waver however. Furthermore, all parts of comets ordinarily disappear when the Sun is eclipsed. The idea that the nucleus consists of a flying gravel bank probably came from such observations. Each individual object would be too small to be detected alone.

However, there are serious objections to this idea. For example, Oort[16] has pointed out that the flying gravel bank is not sufficiently compact to withstand disruption by the Sun's tidal forces. Also the gases released during close passages to the Sun would contribute to this effect, tending to start the array diverging.

Another explanation might be that the nuclei are ordinarily small and not very massive. For example, Lexell's comet of 1770 passed within the orbits of Jupiter's satellites without noticeably disturbing them. In fact, no effect from the gravitational attraction of cometary nuclei has ever been observed on any moon or planet.[16] In general there is no evidence to indicate that the masses of cometary nuclei are great enough to hold an atmosphere of any significant size. Consequently, both the coma and tail must be strictly transitory, requiring a continuous evolution of gases and dust for their maintenance.

From numerous tail and coma observations, it is known that neutral and ionized gases in addition to dust particles are present. The latter conclusion is deduced from the fact that reflected sunlight is partially polarized. The electron density should not be high enough to cause this polarization.

Bright bands of CN and C_2 are seen in the heads although they are missing from the tails, while the emissions from CO^+ and N_2^+ are very strong in the tails.

One might expect CN^+ and C_2^+ in the tails also, but these are spectroscopically invisible since their strong lines lie in the spectral region below 3000 Å. Molecules such as CO and N_2 would not be detected. Molecular recombinations will not tend to radiate at pressures above 0·1 mm. or so, since a third body will ordinarily remove the energy. However, at lower pressures, recombinations which occur for two-body collisions must radiate to remove the excess energy.

The spectra of a few comets which came close to the Sun consisted of a continuous background with a number of intense bands. From the bands it is believed that carbon, sodium and iron dust is present in small amounts. It can be deduced that many other molecules, mostly compounds of hydrogen, oxygen and nitrogen are present in the nucleus. This comes partly from observations of the tails where such radicals as –OH, =NH, $-NH_2$, and $=CH_2$, have been found.

If we now list the more numerous elements in the universe versus their relative abundance[17] it is found that comets are made out of materials which practically head the list. Sixteen elements in the order of decreasing abundance are listed in Table I with hydrogen normalized at 1000.

TABLE I.—*Relative Abundance of Elements in the Universe*

Element	Relative abundance
Hydrogen	1,000
Helium	79
Oxygen	0·5
Neon	0·2
Nitrogen	0·2
Carbon	0·06
Magnesium	0·04
Silicon	0·03
Iron	0·02
Sulphur	0·01
Argon, Fluorine, Sodium, Calcium, Nickel and Aluminium	each ~0·002

Thus it appears that comets consist of the more abundant elements in the universe.

Helium is not expected to be present since it does not exist in the solid state at low pressures. Liquid helium in comets is not feasible either. Hydrogen and oxygen are very reactive and probably are present as solids in combination with other materials. This is in agreement with Whipple's "icy" conglomerate comet model.[19]

According to this hypothesis, the nucleus consists of "ices" with metallic impurities. The most numerous "ices" consist of combinations of hydrogen with carbon, nitrogen, and oxygen. We might expect to find frozen water (H_2O), methane (CH_4), ammonia (NH_3), acetic acid (CH_3COOH), hydrazoic acid (HN_3), hydrocyanic acid (HCN), etc. The "ices" would probably constitute 80% of the mass of the nucleus. This might also help to explain why the nucleus does not appear opaque against the Sun's background. That is, the "ices" would partially transmit the Sun's light and reduce the contrast to the point where the edge of the nucleus was invisible. The inert elements helium, neon and argon would remain uncombined, the latter two frozen solid. It is interesting to note that about 80% of our high explosives are combinations of C, N, O and H. However, comets may store energy by another mechanism, which could, under ideal conditions, make them super high explosives. This brings us to the mechanism which may explain most of the puzzling phenomena associated with comets, the frozen radical and atom.

1. Molecular Fragments

It has been observed in the laboratory that certain radicals and atoms can be isolated and frozen, or stored indefinitely, in a suitable solid matrix, *i.e.*, provided the temperature is low enough. Such materials have been called "frozen radicals," the possibility of frozen atoms being understood. The term "frozen molecular fragment" clearly implies radicals and atoms and is, therefore, a little more general. At room temperature, it is common knowledge that the majority of radicals have very short lifetimes during chemical reactions, *i.e.*, of the order of a few microseconds. The short lifetimes are a consequence of two types of reaction: (1) a radical combining with another radical, or (2) a radical combining with another molecule. The collision efficiency of the first type may be very high, near 100 per cent., whereas the second type obeys the Arrhenius expression for chemical reaction rates, which implies that the rate goes to zero as the temperature is reduced to absolute zero. However, it is possible that the reactions of the first kind may have a zero or small temperature coefficient, even negative. In order to store radicals, therefore, it appears that they must be kept apart by a suitable rigid matrix (an "ice," for example). This has been done in the laboratory for radical concentrations of less than 1 per cent. When the temperature is raised to a critical point, the radicals recombine and give off heat and sometimes light. For example, irradiation of ammonia "ice" (NH_3) at liquid nitrogen temperature (77° K.) by gamma rays produces the frozen radical $-NH_2$ which persists indefinitely until the temperature is raised to a critical value.

Since radicals and most atoms have unpaired electrons, they are paramagnetic and, accordingly, can be identified by a paramagnetic resonance spectrometer.

The low-energy protons from the Sun as well as the ultra-violet light, gamma rays, etc., should be very effective in creating frozen molecular fragments in the nuclei of distant (extremely cold) comets. The attainable ratio of molecular fragments to molecules is not known, but apparently it can become large enough to cause violent explosions, or it may reach a point where the matrix is highly unstable so that the slightest perturbation could set off a chain reaction. Recently, Donn and Urey[19] discussed and attributed the phenomena of cometary outbursts and heating to frozen radical explosions. Urey[20] has also attributed the formation of tektites, which have the appearance of extraterrestrial

bodies, to cometary collisions with the Earth. The tremendous heat of combination would be sufficient to melt portions of the surface, thereby forming tektite regions.

2. Meteoroids

We shall assume that the newer comets, which are not lacking in "ices," have a sizeable percentage of frozen molecular fragments. The amount will evidently vary with the distance from the Sun. It is interesting to observe that the capacity of a comet to act as an energy storage device makes it a refrigerator, in a sense, and, indeed, this may help to keep it preserved during perihelion passages. Much of the stored energy may be reradiated slowly by chemiluminescence. Comets have been observed to give off an excess of light after perihelion passages until they recede to a distance of two or three astronomical units. From time to time local explosions may occur in the nucleus, causing chunks of activated "ice" to be released. Ancient comets which have lost their "ices" by sublimation may eventually become meteor streams or a part of the sporadic influx. We shall use the term meteoroid to designate all of this debris which is associated with comets. In this sense, micrometeoroids would be synonymous with cometary dust, but, since the term micrometeorite is popularly used indiscriminately, we shall use "meteoritic dust" and "micrometeorites" as general terms describing all types of incoming dust. In other words, a micrometeorite need not come from a meteorite, and we shall dispense with the term micrometeoroid. Also, it is assumed that meteorites are not found in comets and materials resembling meteoroids are scarce in the asteroids.

It is evident that there can be two kinds of meteoroids: (1) meteoroids which have a scarcity of metallic impurities, and (2) cindery bodies which have remained and coalesced after the majority of "ices" have sublimated. These latter materials may constitute a part of the sporadic influx. In one quantitative measurement which was made with the Super-Schmidt meteor cameras, a bulk density of 0·05 g./cm.^{3} was determined. This is very low, but, if it is assumed that it was a cindery particle, then the ratio of "ice" volume to dust volume could have been very high, *i.e.*, 90 per cent. or higher depending on the microscopic density of the cinder. From this it would seem that meteoroids which have a scarcity of "ices" are quite fragile. The fact that meteoroids have been observed with radar to disintegrate prior to becoming luminescent may also be an indication of frailty. On the other hand, meteoroids containing a high percentage of "ice" could also disintegrate before becoming luminescent by rapidly sublimating. Or if the "ices" contained enough molecular fragments, a slight atmospheric perturbation might set off a shattering explosion, or possibly a sequence of tiny explosions if the dust content was high enough to act as a damping mechanism. Faint meteors which flare suddenly could be in this latter category also, since recombining radicals or atoms will often give off light. This might be pictured as a cindery particle with an icy interior, the "ice" on the surface having sublimated. Presumably the light from such meteors remains relatively faint until the outer crust or dust covering has eroded enough to expose the nucleus of "ice" and dust.

If meteoroids consist of "ice" and dust, then it is understandable why they have never been found on the ground. Even if they did land and survive the impact, the relatively high ground temperature would soon cause them to melt or sublimate.

IV. METEORITIC DUST

Although the Sun is active in removing meteoritic dust from the solar system, it also assists the comets in releasing new dust, especially during perihelion passages. Whipple[4] estimates that thirty tons of dust per second are released on the average by observable comets in our solar system. Much of this is trapped by the planets but enough remains to supply a part of the zodiacal cloud and the solar corona. Although the Earth receives a share of this dust directly, it gets an additional supply from meteoroids that explode or disintegrate in the atmosphere, and from vaporized meteorites.

It has been known for over a century that meteoritic dust is continually settling to the ground or coming down in rainwater, hail and snow, etc. The red and dark-coloured rain which has been described at different times in the past might be explained on this basis. Attempts have been made to collect meteoritic dust on the ground by several methods, *e.g.*, evaporating rainwater, exposing tacky slides to the sky, inserting magnets in drainpipes, collecting snow or hail, melting glacier ice and filtering ocean sediments. Since the magnetic dust particles are easier to collect and recognize than other types of dust, they have been unduly emphasized, although terrestrial magnetite is a common contaminant. It is just about impossible, however, to make quantitative measurements at ground level. There are several reasons for this, *e.g.*, the time of fall may be several weeks or months and, during this time, the dust tends to be blown over wide areas. Also, terrestrial contaminants are continually blowing back into the atmosphere, so that one wonders if one is dealing with particles that are landing for the first time. Even when the tides go down, the atmospheric dust count increases. Bigg[21] has found that the dust in the first 20 km. above ground is predominantly terrestrial. It would seem, therefore, that quantitative measurements of incoming dust would have to be made above this 12-mile dust layer.

Much of the incoming dust can be correlated with meteor showers. For example, Svesta[22] found that dust appeared in the first 10 km. above sea level from 30 to 40 days after prominent meteor showers, and Zacharov[23] found that dust persisted for 24 days after the Perseid shower. Over a century ago, Cozarri of Padua, Italy, found ashy grey magnetic particles in hail. The date

was 25 August, 1834, or nearly three weeks after the Perseid shower.

1. Bowen's Rainfall Correlation

Recently Bowen[1,2] suggested a correlation between prominent meteor showers and excessive rainfall. He found that rainfall peaks followed meteor shower peaks by 28 to 30 days. Following this, Bowen[3] found additional correlations between snow cover, cirrus cloud and meteor showers. One criticism of Bowen's suggestions has been that there is no sharp discontinuity between the influx of visual sporadic meteors and visual shower meteors. Also, when radar is used to extend the photographic observations of particles in meteor streams to sixth, seventh and eighth magnitude, it is found that there is a paucity of smaller sized particles.[24] If this trend continued down to particles of micrometeorite sizes, then there could hardly be enough dust in the 0·1 to 5 μ size range to supply the tremendous number of freezing nuclei needed for rainfall. The apparent discrepancies can be resolved by showing that a sharp increase in extremely fine debris does occur during prominent meteor showers. To do this, it is assumed that there is an essential difference between shower meteoroids and sporadic meteoroids, *i.e.*, the sporadic meteoroids are believed to be mostly cinders, while the shower meteors are believed to contain a high percentage of "ice" and stored chemical energy. Hence, the sporadic influx from day to day consists of meteorites and fragile cinders which enter meteor altitudes with high velocities. The cinders probably disintegrate and burn without releasing much dust. However, if the meteoroids from prominent showers have a sizeable percentage of frozen radical "ices," then only a small perturbation from the upper atmosphere would be needed to trigger off a chain reaction. The meteoroids would therefore explode or rapidly disintegrate high above meteor altitudes. The dust imbedded in the "ice" matrix would be shattered and dispersed. It is believed that the resulting dust particles would be less than a few microns in diameter, and since this dust is too small to produce visible meteors, there could be a much greater influx during prominent showers than is ordinarily recorded. This would give the necessary discontinuity in the influx which is needed to explain the rainfall and the appearance of dust in the atmosphere following prominent meteor showers. The recorded influx would consist of meteoroids which were lacking in stored chemical energy, such as dust aggregates.

2. Speed and Space Density of Meteoritic Dust

If the foregoing hypothesis is valid, then the range of speeds of meteoritic dust may have wider limits than suspected on the basis of visual meteor data. Johansson and Lungberg[17] have shown that explosives, when detonated *in vacuo*, will produce gaseous explosion products with velocities approaching 21 km./sec. (13 miles/sec.). Since this velocity, within limits, is independent of the high explosive used, it is believed that a portion of the fine dust resulting from meteoroid and cometary explosions will have comparable velocities. If this is correct, then during meteor showers, for example, the very fine dust could have speeds ranging from −10 km./sec. to +93 km./sec. (−6·2 to +58 miles/sec.), the negative sign implying that dust from slower meteoroids could reverse its direction. It may therefore be possible that, at times, a satellite could receive more dust from below than from above.

Even higher velocities could result from shaped charge effects, *i.e.*, from meteoroids containing natural cavities. For example, Koski and co-workers[26] obtained jets from shaped charges with speeds as high as 90 km./sec. by using liners made of beryllium. In general, liners with higher atomic weights exhibited lower jet velocities in chronological order. Velocities of the order of 20 to 70 km./sec. are needed to explain the formation of comet tails on the basis of the "fountain model" hypothesis.[27,28] Theoretically, the ordinary shaped charge explosive will produce jet velocities approaching twice the detonation front velocity when the cone angle is made small.[29] However, if allowance is made for the fact that the explosion gas velocity is a function of air pressure, then with some of the common explosives, a jet velocity approaching four times the detonation front velocity may be possible (about 30 km./sec.), as will be explained shortly.

Under such conditions it is possible that a cloud of meteoritic dust may surround the Earth, since some of the dispersed dust will undoubtedly fall in satellite orbits. This could be one of the principal sources of the Gegenschein or sky glow, since the dust could extend out several thousand miles. This may also tie in with the low frequency electromagnetic whistling effects which have been recorded following lightning flashes. The dust would be a source of electrons because of the photoelectric effect.

In 1956, Berg and Merideth[30] used a new method for detecting micrometeorite impacts. A polished Lucite cone was completely coated with an 800 Å layer of aluminium. The base of the cone was exposed through a hole in the side of an Aerobee rocket to the incoming debris, while the apex was focussed on a 1P21 photomultiplier tube. It was believed that incoming dust would make a pin hole in the aluminium and at the same time give a perceptible light flash. The calibration showed that light pulses as faint as $1{\cdot}5 \times 10^{-12}$ watt-seconds could be readily detected. The rocket was launched from the White Sands Proving Grounds, New Mexico. A constant rate of impact was recorded between 85 and 103 kilometers, the latter altitude being the apex of the trajectory. The influx seemed to be omnidirectional except for the shielding of the Earth. They obtained one impact per cm.^{2} every 57 seconds. The particles were a few microns or less in diameter, and according to Berg[31] incapable of penetrating a Mylar plastic film 0·001 in. thick.

Grimminger[32] made a theoretical study of impact probability and, by extrapolating the visual meteor data

from 5th to 30th magnitude, calculated that an area of 1,000 ft.² would receive 0·215 hits/sec. on the average for particles with diameters of one micron or greater. Thus, the particles encountered by Berg and Merideth were about 76,000 times as numerous as predicted by extrapolation of visual meteorites. If, indeed, these small particles resulted from meteoroid explosions, then there would be no connection between their number and the number of micrometeorites on the basis of extrapolation of meteorites. It is the particles of this size, and the still smaller particles that the the equipment used by Berg and Merideth probably would not detect, that would be expected to cause rainfall a few weeks later. However, the Aerobee rocket was fired on 17 November, 1955, which is the date that the annual Leonid meteor showers begin. If the Leonids released a large quantity of dust, then, according to Bowen's hypothesis, there should be a meteorological effect on 17 December, 1955, or 30 days later. Bigg[33] found that the only sharp peak in freezing nuclei in Western Australia for the month of December was on 16 December, 1955. Another peak occurred about 1 January, 1956, which, according to Bowen[3] can be attributed to the Bielid meteor shower. Arakawa[34] found that one of the dates of maximum snow cover over Japan is 18 December, after having checked weather records covering 323 years of observation. Also, 18 December appears to be a date of maximum cirrus cloud over Western Australia. It would appear, therefore, that the dust encountered by Berg and Merideth may be the best available evidence for the release of large quantities of fine dust during meteor showers.

V. ACCELERATION OF DUST IN THE LABORATORY

Whipple once wrote that cometary dust might be porous, possibly resembling smoke particles. Whether such dust, travelling at speeds of many miles per second could do much eroding on a glass window, for example, is not known. To help answer questions like this, a study of methods for accelerating dust to high speeds was undertaken. Since there were other groups with the same objective, an attempt was made to avoid duplicating their efforts. Thus, *e.g.*, the technique of using a shock tube for accelerating small particles was not studied because the Armour Research Foundation was giving it serious consideration. Another factor in the choice of acceleration methods was the physical size of the accelerator. That is, because of the cost and construction time, the philosophy that only small accelerators should be considered initially was followed. From the viewpoint of making reliable measurements, there were the additional requirements that the physical properties of the test particles should be known beforehand and preserved during the acceleration period, and contaminants of various kinds should not be present. For these reasons, a technique such as the electrically exploded wire did not appear satisfactory.

The idea of levitating a solid *in vacuo* and then spinning it to high angular velocities by an external rotating magnetic field was briefly considered. However, solid iron spheres were observed by Beams *et al.*[35] to fall apart when the peripheral speed reached one kilometer per second, independent of the radius. Some other techniques were examined before it was found that the shaped charge explosive, with some important modifications, appeared to be the most expedient mechanism for producing high speed dust and simultaneously satisfying most of our requirements. An exception might apply to extremely fine dust which could be accelerated to high speeds. Damages could be studied with a microscope.

A drawing of a typical shaped charge explosive is shown in Fig. 2. A charge is said to be "shaped"

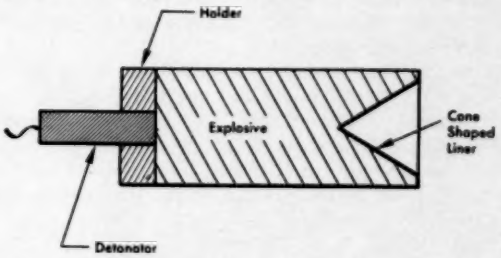


FIG. 2. The essential components of a shaped charge explosive.

when a cavity, such as a cone, is introduced in one end. Charges shaped in this manner give rise to tremendous pressures and penetrating blasts in the region of the cavity. They are used by the military for penetrating targets. Explosives of this type were used by coal miners in the early 1800s, long before the appearance of technical papers on the subject. Later it was found that when a cavity was lined with a metal (for instance, copper), the penetrating properties of the resulting jet were increased considerably. Following this the cone angle was optimized. However, the cone angle which gives maximum penetration does not give maximum jet velocity. The optimum cone angle for maximum jet velocity is zero,[29] *i.e.*, a narrow cylindrical cavity similar to that used by Koski and co-workers.

1. MODIFICATIONS OF THE SHAPED CHARGE

It was felt that the ordinary shaped charge technique had to be modified in order to satisfy our requirements. First of all, an explosive was needed that did not produce solid detonation products. In the second place, the same could be said for the liner. In addition, there were the problems of conveniently mounting identical particles in the liner, and controlling the amount of thermal energy absorbed by the liner and the imbedded particles.

The first requirement can be satisfied by using vacuum-cast Composition B. According to W. A. Allen and B. B. Mayfield[36] of the U.S.N.O.T.S., China Lake, California, this explosive does not produce solid

detonation products at the detonation temperature of 4425° C. It is carbon deficient and oxygen excessive so that all carbon should be converted to CO and CO_2. The high detonation velocity of 7·8 km./sec. is also desirable.

To satisfy the second requirement the liner could be made out of a suitable frozen liquid, *e.g.*, distilled water. This would also satisfy the mounting problem for the test particles could be frozen in place. Heating by conduction would not occur but viscous and radiant heating might be considerable.

In order to prevent the particles from overheating, they could be stained white, whereas the water could be dyed a suitable colour, depending on the amount of thermal energy needed to vaporize the liner. The thickness of the liner could also be varied for temperature control. Some other liquid could be used if the specific heat, and heat of sublimation, etc., were too high for water. The optimum liner thickness and colour would be determined empirically.

If the thermal energy transferred to the liner was enough to vaporize it but not the test particles, then a high-speed stream of gas and dust should result. According to the theory of the shaped charge, the jet velocity should approach twice the detonation velocity for small cone angles. Hence, if the device was fired at atmospheric pressure the initial jet velocity could be near 15·6 km./sec.

However, at these speeds the particles would burn up in air. Since it is necessary to fire the charge into an evacuated chamber, if we are to simulate conditions at high altitudes or in space, there will be no ablation problem. One other effect could occur which is beneficial. That is, Johansson and Ljungberg[25] found that the ejection velocity of explosion gases from the surface of explosives is a function of air pressure. At low pressures the explosion gases approached 21 km./sec. from the end surface of a cylindrical charge. It is possible, then, that dust particles which are mounted in a thin liner on the surface of an explosive will be given comparable velocities when fired into vacua. If this is true, then an enhanced effect might occur from a shaped charge explosive. In order to determine an order of magnitude, the conventional theory of the shaped charge collapse[29] was modified to approximately fit this new picture.

In the conventional theory the explosion pressures are pictured as acting over a finite space and time. This causes the walls of the cavity to swing in a circular arc for a finite time period. It is then found that the maximum jet velocity is independent of the collapse velocity. Since the collapse velocity is of the same order, or less than the detonation front velocity at normal atmospheric pressure, the theory gives good results. In the revised version, the liner is assumed to start collapsing instantaneously, which corresponds to a delta function pressure front. This is an approximation and it represents the other extreme. It leads to a greater maximum jet velocity V_j, *i.e.*,

$$V_j = U_D\left[1+\left(1+\frac{V_0^2}{U_D^2}\right)^{\frac{1}{2}}\right],$$

instead of $2U_D$, where U_D is the detonation velocity and V_0 is the collapse velocity which is absent in the conventional theory. A jet velocity of 30 km./sec. would be the upper limit for Composition B.

Side and top views of a proposed shaped charge testing facility are shown in Fig. 3. The inverted funnels

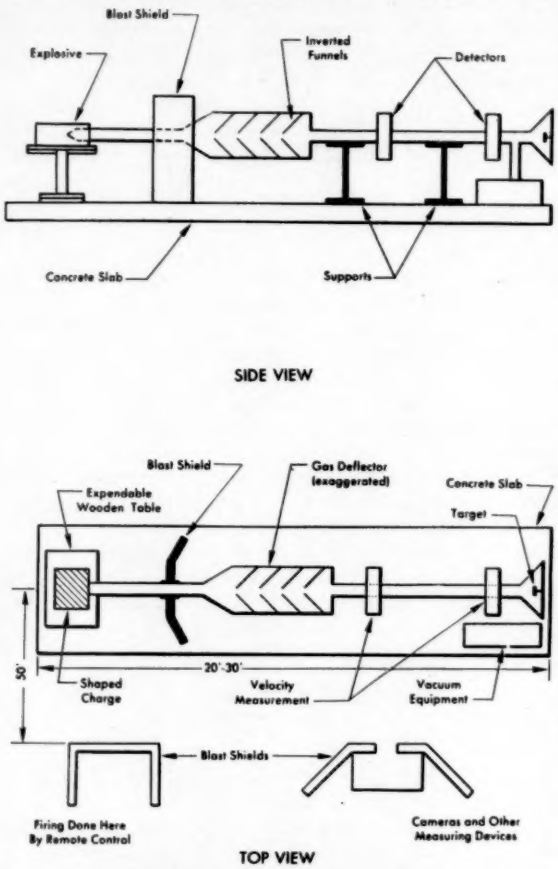


FIG. 3. A proposed facility for accelerating dust particles to high speeds. The modified shaped charge is fired into an evacuated system, which consists, from left to right, of about 5 ft. of expendable glass tubing, a blast shield to protect the remainder of the equipment, a series of inverted funnels which separates much of the explosion gas from the dust particles, velocity-measuring devices, and finally, a target for damage studies

serve to deflect much of the explosion gas in addition to those particles which are not on a prescribed axis. Thus, even if only one or two high-speed particles pass through the detecting equipment, they will be accurately located in space, because of the funnel alignment, and consequently the detection problem is simplified considerably.

VI. SUMMARY

Meteoritic dust is not large enough to produce visual meteors and consequently there is a dearth of information pertaining to the physical properties and speeds.

However, it is believed that the incoming dust which has landed on the ground may have arrived directly, *per se*, or indirectly, from either vaporized meteorites, or disintegrating and exploding meteoroids. The condensed vapour droplets from meteorites yield siliceous (glass-like) and magnetic (iron with a fusion crust) spherules which are formed at altitudes below 75 miles.

Meteoroids which rapidly disintegrate or explode during meteor showers, may generate the very small dust (less than 5 μ in diameter) which is needed to explain the various meteorological phenomena which follow prominent meteor showers in 28 to 32 days. The majority of this dust may have diameters ranging from 0·1 to 5 microns.

Old meteor streams, or meteor streams lacking in "ices" may contain quantities of cindery or sponge-like dust aggregates. Prior to becoming a meteor, such materials may disintegrate because they apparently are fragile, and when ablation starts, the small flecks may be consumed rapidly, yielding various oxides in the form of smoke and gas. It is unlikely that these flecks would form spherules.

The spherules from vaporized meteorites have been found on the ground. They range in diameter from 10 to 250 microns. The siliceous spherules have average diameters of roughly 27 μ and mean specific gravities of 2·8. The black magnetic spherules have average diameters of 44 μ and mean specific gravities of 5·2.

Meteoritic dust with diameters of a few microns was encountered in the experiments of Berg and Merideth at a constant rate between 85 and 103 kilometers. They recorded one hit per cm.^{2} every 57 seconds. The encounters were not violent enough to penetrate a Mylar plastic film of the thickness of 0·001 in., although the individual particles did penetrate an aluminium film which was evaporated on Lucite and which had a thickness of 800 Å (8×10^{-6} cm.). Since these particles were encountered on the first day of the Leonid meteor shower, they could have resulted from meteoroids exploding or disintegrating high in the atmosphere. It is unlikely that the dust was brought in, as such, by the shower, since a meteor stream would not be capable of holding a cloud of dust together in the presence of the Sun's radiation. As predicted by Bowen, meteorological phenomena occurred about thirty days later (rainfall, cirrus cloud and snow cover).

If meteoroids actually do rapidly disintegrate or explode at high altitudes, then there exists the possibility of a dust cloud surrounding the Earth which may extend out for many thousands of miles. A certain amount of gas (radicals and ions) would accompany the dust initially. However, the dust could remain longer in satellite orbits. Both the gas and the dust would be continually resupplied by additional meteor showers.

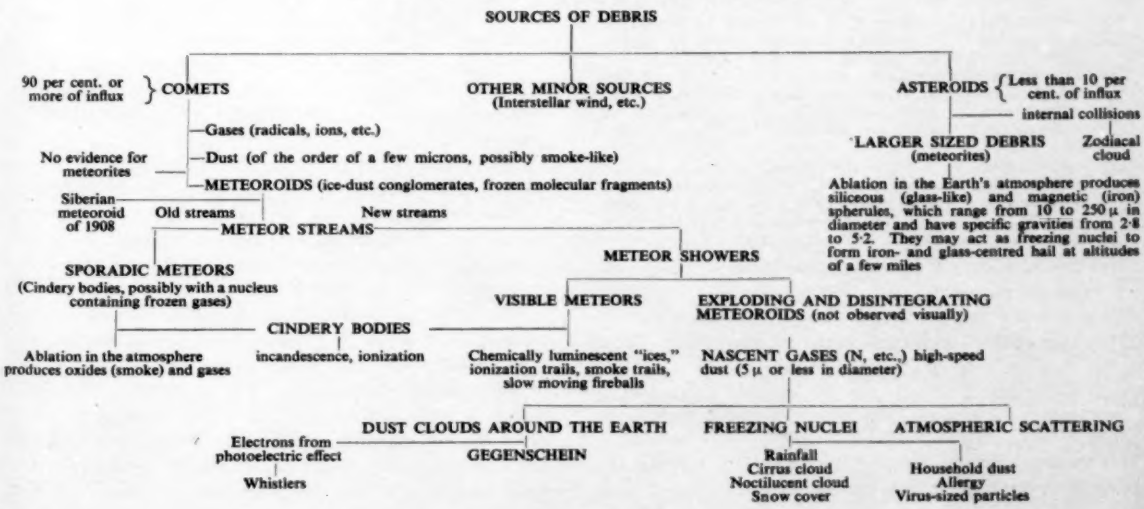
A rough picture of the various dust sources is given in Table II in outline form.

The erosion caused by spherules could be significant over long periods but they can be avoided by flying higher than meteor altitudes. A rocket plane could probably avoid them by flying above an altitude of 75 miles. The possibility of a spherule acting as a freezing nucleus may also exist, so that a fast-moving vehicle might occasionally encounter at low altitudes a hailstone which had a core of stone or iron.

Higher-flying vehicles, such as satellites, may encounter the finer cometary dust even at altitudes of several thousand miles. The extremely fine, abundant dust, which erodes rather than punctures, would not be hazardous. Whipple[37] has estimated that it might frost a glass window in one to two years.

It was noted that dust may be coming from underneath as well as from above satellites.

TABLE II.—*Sources of Interplanetary Debris*


SOURCES OF DEBRIS
90 per cent. or more of influx
COMETS
OTHER MINOR SOURCES (Interstellar wind, etc.)
ASTEROIDS
Less than 10 per cent. of influx
internal collisions
Gases (radicals, ions, etc.)
Dust (of the order of a few microns, possibly smoke-like)
No evidence for meteorites
METEOROIDS (ice-dust conglomerates, frozen molecular fragments)
LARGER SIZED DEBRIS (meteorites)
Zodiacal cloud
Ablation in the Earth's atmosphere produces siliceous (glass-like) and magnetic (iron) spherules, which range from 10 to 250 μ in diameter and have specific gravities from 2·8 to 5·2. They may act as freezing nuclei to form iron- and glass-centred hail at altitudes of a few miles
Siberian meteoroid of 1908
Old streams
New streams
METEOR STREAMS
METEOR SHOWERS
SPORADIC METEORS (Cindery bodies, possibly with a nucleus containing frozen gases)
VISIBLE METEORS
EXPLODING AND DISINTEGRATING METEOROIDS (not observed visually)
CINDERY BODIES
Ablation in the atmosphere produces oxides (smoke) and gases
incandescence, ionization
Chemically luminescent "ices," ionization trails, smoke trails, slow moving fireballs
NASCENT GASES (N, etc.,) high-speed dust (5 μ or less in diameter)
DUST CLOUDS AROUND THE EARTH
FREEZING NUCLEI
ATMOSPHERIC SCATTERING
Electrons from photoelectric effect
GEGENSCHEIN
Whistlers
Rainfall
Cirrus cloud
Noctilucent cloud
Snow cover
Household dust
Allergy
Virus-sized particles

A modification to the shaped charge explosive was discussed which might enable dust to be accelerated to speeds of 10 to 18 miles per second.

ACKNOWLEDGEMENTS

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CORRESPONDENCE

(In these letters to the Editor, some passages have necessarily been omitted)

Design of Step Rockets

SIR,

I was much interested in the graphical construction of the parameters of a step rocket given by Mr. Cross.[1]

Although I understand that it is his aim to get only a general idea of the order of magnitude of these parameters, I think that a slightly different approach to the problem would be more logical.

For the construction of a step rocket usually three data are given, namely:

(1) the characteristic velocity, V, which depends on the nature of the mission—for instance, a circular orbit around the Earth, for which this velocity is about 9·5 km./sec.
(2) the velocity of the exhaust gases, c, for which we choose either the maximum velocity which is technically possible at the present time, or a velocity which we think possible in the future. Say that this velocity is now about 2·5 km./sec.
(3) the structural ratio (s) of the rocket, for which we also choose either the maximum value which is technically possible at the present time, or a value which we think possible in a future time. Let us assume that this value is 5 at the present time.

These are the data from which we must calculate or construct the parameters.

Now V and c give us directly the *overall mass-ratio* (R) of the step rocket, according to the equation:

$$R = \exp(V/c).$$

Thus in our case:

$$R = \exp(9{\cdot}5/2{\cdot}5) = 44{\cdot}7.$$

This value is given by the line $S = \infty$ in Fig. 4 of Ref. 1. Now we must decide how many steps the rocket will contain. If we choose *three* steps, then the *single mass-ratio* (r) of a sub-rocket (assuming that this ratio is the same for all sub-rockets) will be:

$$r = R^{1/n} = 44{\cdot}7^{1/3} = 3{\cdot}55.$$

Now from r and the given value of s the *single payload-ratio* (p) of the sub-rocket (again assuming that s is equal for all steps) is given by:

$$p = r.\frac{s-1}{s-r} = 3{\cdot}55 \times \frac{5-1}{5-3{\cdot}55} = 9{\cdot}78.$$

Then the *overall payload-ratio* P (and *not* the overall mass-ratio!) of the step rocket will be:

$$P = p^n = (9{\cdot}78)^3 = 935 \quad \text{(cf. 1000 for Vanguard)}$$

Starting with s and p, as in Ref. 1, will unavoidably result in a non-integral number of steps, which is impossible.

The number of steps has a dominant influence on the mass of a step rocket.

If we calculate the overall mass-ratio P of a *four*-step rocket with the same data as before, we find: $P = 335$.

Therefore the number of steps (n) should always be mentioned with the overall payload-ratio P.

The graphical construction for P according to this method is as simple as the method described in Ref. 1.

We divide the *total decade velocity* ($V_D = 0{\cdot}434\ V/c$) by the *number of steps* (n). The quotient is the *single decade-velocity*. We set off this value on the abcissa and draw the ordinate at this point. Where this ordinate intersects the line R (or $s = \infty$), we find the *single mass-ratio* (r). Then we calculate or construct (see

Fig. 1) the *single payload-ratio* (p) from r and s, and we mark this value on the ordinate of the single decade-velocity. Then we draw a line through this point and the zero-point of the coordinates. Where this line intersects the ordinate of the *total decade-velocity*, we find the *overall payload-ratio* (P).

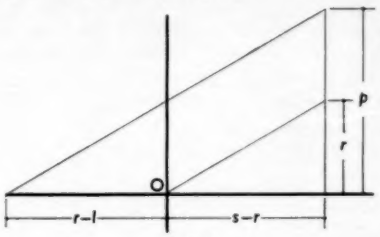

FIG. 1.

I calculated the values of P for the more likely values of c and s, namely for $c = 2{\cdot}5$, and $3{\cdot}0$, and $s = 5$ and 10 (though $c = 3{\cdot}0$ and $s = 10$ is rather optimistic!). The results are given in Table I.

TABLE 1

			$s = 5$			$s = 10$		
V	n	r	p	P	P_C	p	P	P_C
$c=2{\cdot}5$								
9·6	4	2·61	4·37	366	360*	3·18	102	115*
12·8	5	2·79	5·05	3290	2600	3·48	511	520*
13·3	5	2·90	5·52	5120	3500	3·68	675	670
15·1	6	2·74	4·85	13,000	12,000	3·40	1530	1800
15·9	6	2·89	5·48	27,100	20,000	3·66	2400	2500
$c=3{\cdot}0$								
9·6	3	2·91	5·54	170	150	3·69	50	52
12·8	4	2·91	5·57	961	710	3·69	185	180
13·3	4	3·03	6·12	1400	900	3·91	234	230
15·1	5	2·74	4·83	2630	2500	3·39	448	500
15·9	5	2·89	5·45	4800	3500	3·65	648	670

P_C denotes values of P quoted in Table II of Ref. 1.
* There values have been corrected (see adjoining letter from C.A. Cross).

It is seen that for most values the agreement with the results of Mr. Cross is rather good, although some of them show great differences. These were probably cases where the number of steps was halfway between two integers.

Yours sincerely,

M. VERTREGT.

73, Mozartlaan, The Hague, Netherlands.

REFERENCE

(1) C. A. Cross, *J.B.I.S.*, 1957–58, **16**, 148 (*See correction below*).

SIR,

I would like to make the following comments on the above letter from M. Vertregt:

(1) Both M. Vertregt and I have calculated the same quantity, the initial take-off mass divided by the final payload mass. This is not the overall mass-ratio, as I called it, and I agree that it should be called the overall payload-ratio.

(2) I do not agree that Vertregt's approach to the design problem is the better one. Readers will note that he starts by assigning arbitrarily the number of steps in the rocket he will use for a given mission. He then derives the single-stage payload-ratio. Now he himself states that the overall result is very sensitive to the number of steps used, and this means that for each design he must do the calculation for a whole range of step numbers and finally select that step number which gives the nearest approach to the desired single-stage payload-ratio. It seems easier to specify this ratio at the start, and then obtain the number of steps as an explicit (if non-integral) result from a single simple operation.

(3) I should have pointed out in the paper, as I did in the lecture, that a fractional step has a very real physical interpretation. It means that the first or lowest step has a smaller payload ratio than the rest, *i.e.*, the rocket has a starting booster. For a very small fraction of a step, which would involve a ridiculously small booster, the first stage could be given a slightly larger payload ratio than the rest. This all becomes quite clear if the separate stages are marked off on the diagram, when it will be seen that the imposition of a constant value for p is an artificial restriction.

(4) The differences between our numerical results do not seem to me to arise from the use of fractional steps. Examination shows that Vertregt's results are high whenever his single-stage payload-ratios are high, which is only to be expected. Otherwise, the two sets of figures are in very satisfactory agreement. It should be noted that in checking my calculations I have found three small mistakes which are corrected in the table of my results that Vertregt quotes in his letter.

In general, I should like to emphasize that there is no question of serious error on either side, but rather of selection of the most convenient and logical procedure. M. Vertregt's method is undoubtedly necessary for an exact design in which all steps have identical parameters s and p. In practice, however, I think design engineers may find the von Pirquet diagram simpler to use.

I would like to thank M. Vertregt for this interesting and thought-provoking letter.

Yours faithfully,

C. A. CROSS.

284, London Road, Northwich, Cheshire.

CORRECTION

The note on the use of Figs. 3 and 4 on p. 154 of the paper by Mr. Cross (*J.B.I.S.*, 1957–58, **16**, 148) refers the numerical example incorrectly to Fig. 3. The line for $S = 5$, $P = 10$ is found on Fig. 4, and the characteristic velocities should be 0·55 and 1·10 decade velocities instead of the 0·72 and 1·14 quoted.

The Satellite Telescope

SIR,

Mr. Griffin[1] is correct when he suggests that the resolving power of the segmented mirror would not be as good as that of a single mirror, but then I made no claim that it would be. The author appreciated that this would be a matter of some contention and sought the advice of a University engaged in mirror research before writing the paper.[2] The consensus of opinion was that it should be possible to produce a segmented mirror having a cast aluminium alloy lattice, with a resolution superior to that of a single segment, but how far it would fall short of a single mirror could not be ascertained.

With regard to meteor damage, I feel that Mr. Griffin is being optimistic in thinking that the damage would be in the form of a chip. In view of the high velocity of a meteor, it is more likely that the meteorite would disintegrate, and apart from pitting the mirror would produce a very high local temperature with consequent plastic movement. It is the effect of the latter and its effect upon the mirror figure which the author had in mind.

The question of thermal expansions and their effect on telescope efficiency is a very thorny one, but I do not think that an opaque screen a kilometre distant is practicable, desirable, or necessary.

In the design proposed, the outer shell *is* the thermal screen, being internally coated with the material mentioned in order to enhance its efficiency in this respect. This being the case, Mr. Griffin's suggestion that the secondary mirror be supported from it is in contradiction with the accepted principles of the design of hot structures. To avoid the relatively large temperature changes taking place in the shield it is necessary that the secondary be supported directly from the primary mirror. I agree that by bringing the tie members down to the outside diameter of the secondary there will result about a fourfold increase in line-up deviation (for any given temperature rise) compared with maintaining them parallel to, and just inside, the shield. By Mr. Griffin's argument, he would therefore prefer to see the latter arrangement together with the customary radial ties to the mirror itself. However, closer examination reveals this conception to be false. The temperature of the ties is controlled by the quantities of radiation received and re-emitted, the latter quantity being constant for any given tie temperature. The operating variable is the amount of heat received from the inner surface of the shield and this will vary as the inverse square of the distance of the tie from the shields.

It may therefore readily be shown that a structure running along just inside the shield is subject to thermal radiations many orders higher than that of the structure I suggested and this consideration far outweighs the question of optical lever.

Bearing in mind that during an exposure the change in position of the Sun is barely perceptible, there is no reason for believing that a temperature change of the order of 1° C. will in any way be approached. Any slight changes that do occur will be insufficient to impair optical line up (focus excepted), and will be slow enough in rate for the automatic guidance system to overcome. As originally stated, at no time is sunlight permitted to enter the telescope and earthshine only when the telescope is not in use.

I do not think that Mr. Griffin intends that his remarks about the photographic plate and the observer being equally cold to be taken seriously. The former is exposed to space on one side and has a vacuum between it and the control console on the other. The observer on the other hand is sitting comfortably in an air-conditioned cabin!

The question of observer movement is worthy of close attention and Mr. Griffin's figures clearly illustrate this. They have, however, been carried to the point of using ridicule as an argument and serve to show that he is still thinking in terms of an earthbound astronomer.

The degree to which one can remain still when furnished with adequate support is quite remarkable. It has been the author's own experience to be rigidly restrained in an uncomfortable position with one's forehead firmly planted upon a suitable rest and perform a most trying operation requiring close attention with the eyes and delicate manipulation with the fingers. Such a position is readily maintained for periods of up to half an hour or more. There is no doubt that when one is required to concentrate closely, the discomforture is not noticed to anything like the degree that would otherwise be the case. The satellite astronomer will be expected to work in this manner for periods in excess of half an hour, but then he will have two enormous advantages. In the first place he will be resting comfortably (the designer will have to bear this in mind as being of paramount consideration, and with all controls required during exposure within finger reach) and he will be weightless. This latter is the fundamental advantage, as the principal causes of discomforture are muscle fatigue and circulation constraint.

When the astronomer moves a finger through an arc of 2 in. to effect a control, the deviation of the telescope is $\frac{1}{40}$ second of arc, which is within the limits that Mr. Griffin would consider acceptable. The movements of the heart and lungs are primarily that of expansion and contraction and should not therefore be of serious consequence.

In the author's opinion, Mr. Griffin's comments are constructive and sound in principle, but their quantitative effects were grossly exaggerated in order to arrive at the conclusion that he does.

F. A. SMITH.

Bristol Aero Engines Ltd.,
Filton, Bristol.

REFERENCES

(1) R. Griffin, *J.B.I.S.*, 1957–58, **16**, 584.
(2) F. A. Smith, *J.B.I.S.*, 1957–58, **16**, 361.

Reconnaissance Orbits

SIR,

Mr. S. W. Greenwood concludes[1] (third paragraph, page 536) that with a permissible velocity of 6 miles/sec., no recoverable elliptic orbit beyond the Earth's orbit is possible. The basis for this conclusion is illustrated in his Fig. 2.

It appears that this conclusion is not correct, and that the reason for the error is the incompleteness of Fig. 2. The journey time points marked on this figure are restricted to those for which the elliptic path traces out only one revolution, whereas in reality there are an infinite number of journey times consisting of whole numbers of years in any interval along the curve. For example, between the journey time point marked "1" and the journey time point marked "2" there would certainly be a point corresponding to "1½" if we were not restricted to whole numbers of years. This point would represent the condition where the elliptic path traces out one revolution while the Earth circles the Sun one and one-half times; but it also represents the condition where the elliptic path traces out two revolutions while the Earth circles the Sun three times. Thus, this point would be marked "3," or in fact any multiple of "3." Continuing with this line of reasoning, it follows that in any interval along the curve, there are an infinite number of journey time points consisting of whole numbers of years. From this it would appear that the conclusion drawn in the paper is unwarranted.

Very truly yours,

ALFRED A. ADLER.
Missile Scientist, Space Flight Division.

Bell Aircraft Corp.,
Post Office Box One, Buffalo 5, N.Y., U.S.A.
11 *December*, 1958.

REFERENCE

(1) S. W. Greenwood, *J.B.I.S.*, 1957–58, **16**, 534.

SIR,

Mr. Adler is quite correct. My statement[1] that no recoverable elliptic orbit beyond the Earth's orbit is possible with a permissible velocity increment of 6 miles/sec. depends on the assumed restriction that a single elliptic path is traced out. Points on Fig. 2 lying between the points plotted correspond to elliptic paths that are traced out more than once. Of these additional points, those corresponding to the tracing out of two elliptic paths (with single path times of 1½ years, 2½ years, etc.) are the most favourable from the viewpoint of total journey time (3 years, 5 years, etc.). Other points in a given interval correspond to progressively greater total journey times as the boundaries of the interval are approached.

Solutions may also be found for elliptic orbits within the Earth's orbit other than those given by the restriction that the number of revolutions of the Sun made by the Earth is one less than the number of elliptic paths traced out.

In both the above cases, however, the journeys corresponding to points selected for plotting in the paper appear to have some advantages over those which correspond to points on the curves in the intervals between the plotted points, in that the total journey times are lower. Nevertheless, where the limiting factor is the permissible velocity increment, it may be necessary to accept the longer journey times, and I am grateful to Mr. Adler for pointing out that my restrictions on the possibilities of recovery were unjustifiably severe.

I would like also to take this opportunity of making a correction to Section 2 of Appendix I of the article. The specified velocity requirement of $x = 1{\cdot}83$ miles/sec. corresponds to escape from a circular orbit around the Earth at an altitude of 1000 miles, and not 2000 miles as stated. The value at an altitude of 2000 miles would be $x = 1{\cdot}66$ miles/sec. As pointed out in the Appendix, variations in the altitude selected in the vicinity of the Earth have a relatively small effect on the results, but it is nevertheless desirable to associate a particular velocity requirement with the right altitude, and the author apologises for this slip.

S. W. GREENWOOD.

Department of Aircraft Propulsion,
The College of Aeronautics, Cranfield, Bletchley,
Bucks.
8 *January*, 1959.

REFERENCE

(1) S. W. Greenwood, *J.B.I.S.*, 1957-58, **16**, 534.

CORRESPONDENCE AND SHORT COMMUNICATIONS

The Editor welcomes letters intended for publication, whether they contain comment on material which has appeared in the *Journal* or deal with other subjects of astronautical interest. Short communications, up to 1500 words in length, are also needed.

Prompt publication is facilitated if two or three copies of each letter or communication are provided and the manuscript is a typescript or in a similar form. Communications should be addressed to:

The Editor,
Journal of the British Interplanetary Society,
31 Brooklands Gardens,
Hornchurch, Essex, England.

NEWS AND ANNOUNCEMENTS

B.I.S. NEWS

Mr. R. A. Smith

Council deeply regrets to record the death on 4 February, 1959, after a period of illness, of Mr. Ralph Andrew Smith, Chairman of Council 1956-57, and Vice-Chairman from 1953 to 1956. Mr. Smith was one of the earliest members of the Society and his many talents (not least his eloquence and great skill with pencil and brush) have contributed much to the advancement of our organization and to the development of astronautics. It is no slight to his memory to say that he would have been capable of even greater contributions, had circumstances permitted it—as long ago as 1947 he submitted proposals for experiments with man-carrying rockets to the Ministry of Supply, but the possibility of the government approving a British spaceflight programme at that time was far less than it is now.

An obituary notice will appear in a subsequent issue of the *Journal*.

Council

Dr. G. S. Brosan, who was elected to the Council in July, 1958, recently resigned.

Council has decided to fill the resulting vacancy by co-opting the candidate at the last election who received the next highest number of votes (Dr. W. R. Maxwell). We are glad to announce that Dr. Maxwell has agreed to serve; he is well known to many members of the Society from his work at the Rocket Propulsion Department, Westcott.

Editor of *Spaceflight*

Mr. Patrick Moore, who has been Honorary Editor of *Spaceflight* since it began publication in 1956, has been forced to resign this position because of the pressure of other work. Council has received Mr. Moore's resignation with regret, and desires to place on record its appreciation of his great service to the Society in guiding our new publication through its important early years. We are pleased to say that Mr. Moore will still be able to serve as a Member of Council and has offered to assist the new editor during his first few months of office.

Mr. K. W. Gatland, Vice-Chairman of Council, has been appointed Editor of *Spaceflight* with effect from 7 February, 1959.

Fourteenth Annual General Meeting

The notice and agenda for this year's Annual General Meeting will be printed in the next issue of the *Journal*. Among the items on the agenda will be resolutions embodying the Council's proposals for revising the membership structure of the Society.

The date chosen for the Annual General Meeting is **17 July, 1959**. Members are reminded that notice of nomination for election to the Council is required at least twelve weeks before the date fixed for the meeting. The nomination forms, which are obtainable from the Secretary, must therefore be completed and returned by **24 April, 1959**.

Yorkshire Branch Secretary

Mr. M. C. Greenwood has resigned his position as Secretary of the Yorkshire Branch, and Miss Dorothy Dyson, M.P.S. (5, Harker Terrace, Old Road, Stanningley, Pudsey, Yorkshire) has been appointed Branch Secretary in his place.

Election of Members

The following elections were made at the Council Meeting on 6 September, 1958:

Fellows.

Richard Austin Anderson, B.Sc., Ph.D., c/o Physics Dept., University of Western Australia, Nedlands, Western Australia.
Brian Robert Broom, 151, Northchurch Road, Islington, London, N.1.
Vivian Davies, L.D.S., R.C.S., Brynhall, Newtown, Montgomeryshire.
Felix Dominicus Joseph Dukstra, c/o Zanen Verstoep N.V., Postbox 149, Baghdad, Iraq.
Lt.-Col. Roy Carruthers Fletcher, A.F.R.Ae.S., TERA, Ty Cross Camp, Anglesey.
Frank Girling, B.Sc., Ph.D., 1967, Victoria Park Avenue, Apt. 6, Scarborough, Ontario, Canada.
Hymie Gordon, B.Sc., M.B., Ch.B., M.D., London House, Guilford Street, London, W.C.1.
Gerald Seymour Hunter Lock, B.Sc.(Eng.), 65, Kentmere Avenue, Newcastle-on-Tyne, 6.
William Michael Long, Grad.I.Mech.E., 2, Plumpton Avenue, Blackpool, Lancashire.
Stanislaw Adam Milianowicz, 41, Farm Close, Oakdale, Monmouthshire.
Gouvind Ballabh Pant, M.A., M.S., Dept. of Mathematics, National Defence Academy, Khadakvasha, Poona, India.
Gwilym James Richards, B.Sc., Ph.D., A.R.C.S., The Elbury, Ray Park Avenue, Maidenhead, Berkshire.
K. N. Satyendra, M. S., Ph.D., Westinghouse Electric Corporation, 5050, State Road, Drexel Hill, Pennsylvania, U.S.A.
Alan Ernest Wilmshurst, B.Sc., 136, Whitley Road, Eastbourne, Sussex.
Professor Wendall Haslam Wiser, B.S., Ph.D., L.D.S., Church College, Franklin Junction, New Zealand.
Derek Wood, B.Sc., 88, Mattson Road, Apt. 4B, Downsview, Ontario, Canada.
Wilfred Joseph Woods, B.Sc., 14, Great Ormond Street, London, W.C.1.

Members.

Edward Hugh Baker, c/o Keene Trailer Park, 3126, Spring Street, Paslo Robles, California, U.S.A.
Colin David MacArthur, Mossbank, Linksfield Street, Leven, Fife, Scotland.
Gordon Lewis Blake, 58, Kidbrooke Grove, Blackheath, London, S.E.23.
Richard Collins, 128, Bingham Avenue, Toronto 13, Ontario, Canada.
Michael John Daniel, B.A., 4420, West 5th Avenue, Vancouver, British Columbia, Canada.
Robert Stone Duggan, M.S., 1299, Mayfair Drive, N.E., Atalanta 5, Georgia, U.S.A.
Frank Haller, 50, Overdale Road, Derby.
Edmond Hamilton, RD2, Kinsman, Ohio, U.S.A.
Edward Gordon Harris, 95, Kennington Road, Oxford.
Harry Bertram Hart, 38, Hogarth Road, London, S.W.5.
Duncan Macrae Henderson, c/o Mackay, 16, Rochester Terrace, Leeds 6, Yorkshire.
Irwin Hersey, M.A., 106, West 69th Street, New York, N.Y., U.S.A.
Stephen Jackson, F.R.A.S., Upway, Rode, Bath, Somerset.
Louanne Adams Kalvinskas, B.S., 145, South Wilson Avenue, Pasadena, California, U.S.A.
Lorren Gustaf Larwood, 340, Girdle Road, East Aurora, N.Y., U.S.A.
Foo Mun Lo, 2, Hamilton Road, St. Catherines, Lincoln.
Howard Andrew MacEwen, 86, Fremont Street, Battle Creek, Michigan, U.S.A.
John Bruce Miller, c/o Bye, Nypeueien 8, Nordberg, Oslo, Norway.
Paul North, 5, Keats Road, Coventry, Warwickshire.
Ian Leslie O'Brien, 42, Darnich Road, Sutton Coldfield, Warwickshire.
Eric Parkinson, A.C.C.S., 18, Abbeyville, Blackpool, Lancashire.
Dennis Charles Parsonage, 139, Hales Crescent, Smethwick, 41, Staffordshire.
Edward T. Pitkin, M.S., 26, Calvin Avenue, Reseda, California, U.S.A.
Gerald Bryan Robinson, Steyne Cottage, Mill Road, Angmering, Sussex.
Edward James Rossiter, 43, St. Edward Street, Newport, Monmouthshire.
Stanley Frederick Sarner, B.Sc., 1809, Crest Hill Avenue, Cincinnati 37, Ohio, U.S.A.
Robert Nels Shelgren, B.S., 109, Hallock Street, Jamestown, New York, U.S.A.
George Royden Stevenson, 71, Lichfield Road, Burntwood, Nr. Lichfield, Staffordshire.
Douglas Edgar Stopford, 20, Oakland Avenue, Oxney, Watford, Hertfordshire.
Arne Herbert Svensson, 18, Ridgway, Berkhamstead, Hertfordshire.
Kjell Thommessen, Sofiesgt, 16, Oslo, Norway.
Anthony James Chapman Wakefield, 18, Belmont Road, Sale, Cheshire.
Leslie Warburton, 177, Pitville Avenue, Mossley Hall, Liverpool, 18.

The following elections were made at the Council Meeting on 4 October, 1958:

Fellows.

Ronald Vincent Baughan, 4, Hurst Green Road, Hurst Green, Oxted, Surrey.
Norman William Bridge, B.Sc., 5, Victoria Road, West Kirby, Wirral, Cheshire
George Cairns, 29, O.M.Q., R.A.F., Driffield, E. Yorkshire.
James Rixon, B.Sc., 1, Bankside, Little London, Warrington, Lancashire.
Marinus Vertregt, 73, Mozartlaan, The Hague, Netherlands.
Frank Brierley Whitfield, B.Sc., 6, Gregory Avenue, Croydon, Sydney, N.S.W., Australia.

Senior Member.

Bert Lewis, 47, Queensway, Ashton on Ribble, Preston, Lancashire.

Members.

Esten Julius Atwood, B.A., 220, South Wilton Place, Los Angeles 5, Calif., U.S.A.

NEVILLE ANTHONY BLACK, Blacker House, Caltech., Pasadena, Calif., U.S.A.
JOHN MICHAEL BUTTERWORTH, 53, Charlton Road, Shepperton, Middx.
BONNIE JEAN CHANGSTROM, B.S., 1275 So. Oakland Ave., Pasadena, Calif., U.S.A.
PHILIP SYDNEY DEARDEN, 51, Fredora Avenue, Marton, Blackpool, Lancashire.
JOHN FRANK DETLEF, M.A., 17391, Locust Drive, Los Gatus, Calif., U.S.A.
MICHAEL JOHN GALLOWAY, 128, Stockport Road, Ardwick, Manchester, 12.
CHRISTOPHER LUDWIG GARVIE, 26, Shepherds Hill, Highgate, London, N.6.
RICHARD OLIVER GOODCHILD, 77, Beattyville Gardens, Ilford, Essex.
JOHN ROBERT HUTTON, "Coed-Bel," Lubbock Road, Chislehurst, Kent.
DIETRICH E. KOELLE, Stuttgart-Zuffenhausen, Neuensteinerstrasse 19, Germany.
PETER LEONARD LEBENSOLD, 3160, Ramsay Place, Montreal, Quebec, Canada.
PATRICK MOZELY MARTIN, Stonnington, Christchurch Road, Virginia Water, Surrey.
CHARLES LEWIS SCARBROUGH, B.Mech.E., 3909 9th Avenue, S.W. Huntsville, Alabama, U.S.A.
JOHN WILLIAM SHELDON, B.S., 1468, Highland, Lakewood, Ohio, U.S.A.
JEAN ARMAND VANDENKERCKHOVE, M.S., 283, Avenue Louise, Bruxelles, Belgium.

The following elections were made at the Council Meeting on 1 November, 1958:

Fellows.

ROLAND BENBOW, 58, Tybenham Road, Merton Park, London, S.W.19.
THOMAS ANTHONY BLACKBURN, B.Sc., 91, Cheadle Road, Cheadle Hulme, Cheshire.
DAVID GEORGE CHILD, "Toxteth," Main Road, Marchwood, Hampshire.
STANLEY BRUCE CHURCH, "Woodlands," 110, Gregson Lane, Hoghton, Nr. Preston, Lancashire.
RAYMOND GIBSON CRUDDACE, B.Sc., 75, Ringway, Southall, Middlesex.
DONALD WILLIAM DREISIG, B.S., 4095 E 123 Street, Cleveland 5, Ohio, U.S.A.
SHEILA MARY LEONARD, B.Sc., 121, Lichfield Road, Shelfield, Nr. Walsall, Staffordshire.
JAMES WILSON MEIER, B.E., 7751, Mapleway Drive, Olmsted Falls, Ohio, U.S.A.
FRANK LINTON MORRIS, B.Sc., Officers' Mess, 47GW Regt(Fd) R.A., Haig Lines, Crookham, Hampshire.
JACK MOSHMAN, M.A., Ph.D., Corporation for Economic and Industrial Research, 1200, Jefferson Davis Highway, Arlington 2, Va., U.S.A.
JAMES WILLIAM PARK, G.I.E.E., 213, Restons Crescent, London, S.E.9.
ROBERT ERROL ROBERSON, M.S., Ph.D., 1100 N. Ceritos Drive, Fullerton, Calif., U.S.A.
AUSTIN N. STANTON, M.S., Box 638, Garland, Texas, U.S.A.
WILLIAM JOSEPH TUCKER, Ph.D., D.Sc., 45, Penshurst Avenue, Sidcup, Kent.
GILES ARTHUR KELK VINCENT, 8, Scott Road, Tilgate, Crawley, Sussex.
RALPH EFFNER WHITE, 1699, East Mendocino Street, Altadena, Calif., U.S.A.

Members.

JOHN ALLSEBROOK, 23, Northumberland Street, St. Anns Well Road, Nottingham.
ARMANDO ARMENGOL, Calle 62, No. 1710, Marianao, Habana, Cuba.
PETER JOHN HARTWELL BAILY, 58, St. Albans Road, Halifax, Yorkshire.
ROY WILLIAM DIXON BISHOP, 53, Horsley Road, Kilburn, Derbyshire.
SAMUEL BROWN, 13, St. Leonard's Road, Lanark, Scotland.
HELEN ANGELA CROOME, B.A., 14, The Paragon, Blackheath, London, S.E.3.
JAMES THOMAS DUDLEY, Awarua Radio Station, Private Bag, Invercargill, New Zealand.
THOMAS PETER DUKES, 1, James Avenue, Marton, Blackpool, Lancashire.
SIMON FAIRTHORNE, "Kirk Michael," Clockhouse Road, Farnborough, Hampshire.
WYATT CHRISTOPHER JOHN GATES, Debnershe, Shalford, Guildford, Surrey.
BRIAN ERIC HALL, 72, Brook Road, Cricklewood, London, N.W.2.
MICHAEL HAYWARD, 28, Harewood Road, Isleworth, Middlesex.
EDWARD CAMERON KIRBY, B.Sc., Collyweston, Stamford, Lincolnshire.
ERIC JOSEPH LEVER, "Bristowe," Boyes Drive, Muizenberg, C.P., South Africa.
FREDERICK ANTHONY MARTIN LILL, 180, North Hyde Lane, Southall, Middlesex.
IAN ROBERT MAXWELL, 4, Fitzroy Square, London, W.1.
ANGUS ARTHUR MCKENZIE, Olympic Sound Studios, Carton Street, London, W.1.
GWENDOLEN CAMPBELL MELHUISH, 26, South Road, Brighton Beach, Victoria, Australia.
ROBERT JAMES OPENSHAW, 43, Harcourt Road, Altrincham, Cheshire.
RICHARD STEPHEN SVOREC, 824-36th Street, South Bend 15, Indiana, U.S.A.
JAMES FRANCIS SWALE, B.A., Rudfarlington, Knaresborough, Yorkshire.
WILFRED JAMES WALLACE, Sgts. Mess, R.A.F. Feltwell, Thetford, Norfolk.
ROBERT DOUGLAS WHITTALL, 25, Fernley Road, Mile-End, Stockport, Cheshire.
DAVID HERBERT YOUNG, A.M.I.Mech.E., 118, Abbotsbury Gardens, Eastcote, Pinner, Middlesex.

The following elections were made at the Council Meeting on 6 December, 1958:

Fellows.

PETER WILLIAM BOTHWELL, M.B.Ch.B., D.P.H., 13, Wyndham Way, Oxford.
NORMAN HAROLD COOPER, Grad.I.Mech.E., 36, Manor Park, Silkstone, Nr. Barnsley, Yorkshire.
CHARLES THOMAS HAMMOND, 101, Strathyre Avenue, Norbury, London, S.W.16.
IRWIN HERSEY, M.A., 106, West 69th Street, New York, 23, N.Y., U.S.A.
GEOFFREY THOMAS HIGGINS, 14A, Church Road, Burgess Hill, Sussex.
ANGUS JAMES MCDONALD, Department of Flight, College of Aeronautics, Cranfield, Buckinghamshire.
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JOINT ACTIVITIES

Tenth International Astronautical Congress

The Tenth International Astronautical Congress, which is being organized by the Society, will be held in London during the week **31 August to 5 September, 1959** at Church House, Westminster. It will be the most important international meeting on space research and astronautics to be held this year and an attendance of several hundred is expected. Delegates from all the principal astronautical societies throughout the world will be present and during the course of the Congress the business and committee meetings of the International Astronautical Federation will be held.

Prospective authors are invited to submit papers for presentation through the regional screening committees set up by the Member-Societies of the I.A.F. Each author is asked to notify the Chairman of the appropriate committee of his intention to submit a paper, and should accompany this notification with a title and abstract (50-200 words) in triplicate (three copies), sent to reach the screening committee not later than **1 May, 1959**. If the subject is approved, three copies of the manuscript should be sent in the same way by not later than **1 June, 1959**. These dates must be adhered to, to give adequate time for preparation of preprints.

Further particulars, together with details of the arrangements for registration and particulars of the various activities during the week (which will include works visits, an excursion, tours for ladies and guests, evening receptions, and a concluding banquet at the Dorchester Hotel) will be given in a *Congress Information Bulletin*, to be issued at intervals to those who request it. Application should be made to **The Secretary, B.I.S., 12 Bessborough Gardens, London, S.W.1** (Telephone: TATe Gallery 9371).

Offers of assistance from members willing to act as stewards, couriers, etc., will be welcome; a particular request is made for those capable of acting as interpreters.

Commonwealth Spaceflight Symposium

The I.A.F. Congress announced above will be preceded by a two-day symposium. The theme of this will be Commonwealth Co-operation in a Spaceflight Programme, and it will include communications from the British, South African, Canadian and Indian societies, contributions from B.I.S. members in other Commonwealth countries and a general discussion. Persons resident outside the Commonwealth will be allowed to attend as observers. The Symposium is to be held at Church House, Westminster, on Thursday and Friday, **27-28 August, 1959**. Further particulars will be announced in a later issue of the *Journal* and may also be obtained from the Secretariat.

PERSONAL NOTES

Dr. O. P. MEDIRATTA (FELLOW) has left Short Brothers and Harland Ltd. and is now Deputy Director (Research and Development), Directorate of Technical Development and Production (Air), Ministry of Defence, Government of India, New Delhi.

Mr. E. T. B. SMITH (MEMBER OF COUNCIL) has been appointed to the British Joint Services Mission, Washington, D.C.

ASTRONAUTICAL ABSTRACTS

Edited by J. HUMPHRIES and J. FOLEY

1959–60 PART 1

Beginning with this issue a new classification is to be used; this is given below. Only the main section headings will be given but the order of the abstracts will be according to the sub-headings below.

A list of abbreviations of titles of journals was included with the 1954 index and addenda have been published in subsequent issues of the *Journal*.

1. **ASTRONOMY.**
- ·1 General.
- ·2 Stars and nebulae.
- ·3 Solar system, general.
- ·4 Sun.
- ·5 Planets (except Earth).
- ·6 Earth.
- ·7 Moon.
- ·8 Meteors.
- ·9 Comets.

2. **PHYSICS.**
- ·1 General.
- ·2 Gravitation.
- ·3 Sound.
- ·4 Light.
- ·5 Heat and thermodynamics.
- ·6 Electricity and magnetism.
- ·7 Cosmic radiation.
- ·8 Other upper atmosphere phenomena.

3. **CHEMISTRY AND MATERIALS.**
- ·1 General.
- ·2 Chemistry.
- ·3 Metallurgy.
- ·4 Refractories.
- ·5 Plastics.
- ·6 Miscellaneous.

4. **BIOLOGY AND MEDICINE.**
- ·1 General.
- ·2 Botany.
- ·3 Zoology.
- ·4 Anatomy and physiology.
- ·5 Hygiene and preventive medicine.
- ·6 Pathology.
- ·7 Psychology.

5. **AVIATION AND AERODYNAMICS.**
- ·1 General.
- ·2 Aerodynamics.
- ·3 Rocket-propelled aircraft.
- ·4 Test facilities.

6. **ASTRONAUTICS.**
- ·1 General.
- ·2 Artificial satellites.
- ·3 Lunar and planetary probes.
- ·4 Spaceships.
- ·5 Interstellar and intergalactic travel.
- ·6 Orbits.

7. **PROPULSION.**
- ·1 General.
- ·2 Ram-jets and air-breathing engines.
- ·3 Chemical rockets, general.
  - ·1 Solid propellent.
  - ·2 Liquid propellent.
  - ·3 Propellents.
- ·4 Nuclear rockets, working fluid.
- .5 Ion rockets.
- .6 Miscellaneous.

8. **MISSILES.**
- ·1 General.
- ·2 Short-range.
- ·3 Ballistic.
- ·4 Upper atmosphere research.
- ·5 Trajectories.
- ·6 Guidance and control.

9. **RADIO AND ELECTRONICS; COMMUNICATIONS.**

10. **PLANETARY ENGINEERING.**

11. **BIOGRAPHY AND HISTORY.**

12. **SPACE LAW; SOCIOLOGY.**

13. **MISCELLANEOUS.**

2—PHYSICS

(1) **Some physical properties of pure liquid ozone and ozone-oxygen mixtures.** A. C. Jenkins and F. S. Di Paolo. *J. Chem. Phys.*, **25**, 296–301 (*Aug.*, 1956). (18 *refs.*)

(2) **Satellites for checking Einstein's relativity theory.** M. Subotowicz. *Missiles and Rockets*, **2** (2), 57–9 (*Feb.*, 1957). Describes experiments in which satellites could be used, including the effect of gravity on transmitter frequency, and the changed time scale in the satellite due to its velocity. (7 *refs.*)

(3) **Lyman-alpha intensity and solar limb darkening from rocket spectrograms.** S. C. Miller, R. Mercure and W. A. Rense. *Astrophys. J.*, **124**, 580–5 (*Nov.*, 1956).

(4) **Heat transfer characteristics of RFNA.** *Bell Aircraft Corp. Rocket Engine Dept. Report* 117–982–004, 140 pp. (*July*, 1956).

(5) **Determination of air density at high altitude by means of an Earth satellite.** G. Leitmann. *Am. J. Phys.*, **25**, 115 (*Feb.*, 1957). Derivation of an equation applicable to near-circular orbits. (1 *ref.*)

(6) **Upper atmosphere research.** *Engng.*, **183**, 286 (*March* 1, 1957). A review of the Royal Society's I.G.Y. programme which is to include determination of upper air temperature, wind distribution, origin of the night air-glow and investigation of radio transmission through the ionosphere.

3—CHEMISTRY AND MATERIALS

[*See also* abstracts nos. 1, 28, 29.]

(7) **Storage and handling of unsym-dimethylhydrazine**. *Westvaco Chlor-Alkali Div. Food Machinery and Chem. Corp.*, 16 pp.

(8) **Steady-state burning of a liquid droplet. II. Bipropellant flame**. *J. Chem. Phys.*, **25**, 325–331 (*Aug.*, 1956). (11 *refs.*)

(9) **Problems and promises of liquid ozone**. *Missiles and Rockets*, **2** (1), 61 (*Jan.*, 1957). A short report of the proceedings at the Chicago International Ozone Conference.

(10) **Metal congress tells of new materials**. *Missiles and Rockets*, **2** (4), 44–8 (*Apr.*, 1957). Report of Los Angeles Congress of the American Society for Metals discussions on titanium.

(11) **Structural materials for missile applications at very high temperatures**. J. R. Kattus. *Jet Propulsion*, **27** (6), 644–9 (*June*, 1957). Compares short-time mechanical properties of copper, iron, molybdenum, tantalum and graphite up to their melting points or 5200° F. (2870° C.). Tantalum and molybdenum have appreciable strength between 4000° and 5000° F. (2200° and 2760° C.), but oxidize much more rapidly than graphite, which maintains its room temperature strength to over 4000°F. (2200° C.). In addition to mechanical and chemical properties, thermal properties must be considered in choosing materials for missile structures.

(12) **Time-temperature relationships for rupture of metals in combustion atmospheres**. E. W. LaRocca. *Jet Propulsion*, **27** (6), 674–5 (*June*, 1957). (5 *refs.*)

(13) **Rocket refractories**. H. B. Porter. *Naval Ordnance Test Station Report No.* 1191, 48 pp. (26 *Aug.*, 1955).

(14) **Crystalline "glass" noses into missile field**. *Jet Propulsion*, **27** (6), 690 (*June*, 1957). Item on development of "Pyroceram" high temperature materials.

5—AVIATION AND AERODYNAMICS

(15) **Heat transfer to satellite vehicles re-entering the atmosphere**. N. H. Kemp and F. R. Riddell. *Jet Propulsion*, **27** (2), 132–7, 147 (*Feb.*, 1957). Applies a theory of heat transfer at high velocities and altitudes to the re-entering satellite vehicle. Presents numerical results and analyses the value of aerodynamic lift during descent. (8 *refs.*)

(16) **Hypersonics rubs rough edges off space flight**. *Jet Propulsion*, **27**, 548–50 (*May*, 1957). Survey of facilities and work in progress.

(17) **Stagnation point heat transfer in dissociated air flow**. J. A. Fay, F. R. Riddell, and N. H. Kemp. *Jet Propulsion*, **27** (6), 672–4 (*June*, 1957). (10 *refs.*)

(18) **An extension of the theory of the optimum burning program for the level flight of a rocket-powered aircraft**. A. Miele. *Purdue Univ. School of Aeronautical Engng. Rept. A*–56–1, 49 pp. (*June*, 1956).

(19) **Manned rocket bombers underway**. E. Bergaust. *Missiles and Rockets*, **2** (2), 20–1 (*Feb.*, 1957). Describes a long-range bomber for use in a skip-trajectory of the type proposed by Sänger.

(20) **Upper bounds and conservative estimates for aerodynamic heating at great altitudes**. J. F. Vandrey. *Jet Propulsion*, **27**, 522–6, 546 (*May*, 1957). A simple method assuming most unfavourable flow conditions. In non-marginal cases, such as the launching vehicle for a satellite, the upper bounds result in skin temperatures low enough to serve as a guide for design purposes (4 *refs.*)

(21) **Brief view of hypersonic research**. *Jet Propulsion*, **27** (1), 55 (*Jan.*, 1957). Notes on methods of generating high Mach numbers for missile research.

(22) **Supersonic vehicle of commerce . . . supersonic weapon of war**. J. W. Crowley. *Interavia*, **12** (1), 46–8 (*Jan.*, 1957). Describes aerodynamic research methods for use at speeds up to $M = 20$.

(23) **Aeroballistic forecasting at Redstone**. *Missiles and Rockets*, **2** (2), 86 (*Feb.*, 1957). Description of the Aeroballistics Laboratory of the U.S. Army Ballistic Missile Agency.

(24) **Plasma jets give minutes of I.C.B.M. re-entry heat**. *Missiles and Rockets*, **2** (3), 29 (*Mar.*, 1957). History and present interest in stabilized arc method of obtaining temperatures of 10,000° K. and flow velocities to March 10.

6—ASTRONAUTICS

[*See also* abstract no. 2.]

(25) **Seventh I.A.F. Congress stresses co-operation**. A. G. Haley. *Jet Propulsion*, **27** (1) 60, 62 (*Jan.*, 1957). An account of the congress.

(26) **Need for space-flight know-how**. G. W. Hoover. *Missiles and Rockets*, **2** (1), 47–8 (*Jan.*, 1957). Points out the progress made towards space-flight, emphasizes the need for more knowledge in all the relevant sciences.

(27) **Spaceman Col. Bill Davis**. *Missiles and Rockets*, **2** (6), 103–6 (*June*, 1957). Interview in which Col. Davis gives his views on rockets and spaceflight.

(28) **On the slowing down of time**. C. Casci and B. Bertotti. *Jet Propulsion*, **27** (6), 665–6 (*June*, 1957). Discusses relativistic time dilatation.

(29) **Time dilatation in space flight**. H. G. Loos. *Jet Propulsion*, **27** (6), 665 (*June*, 1957).

(30) **Vanguard instrumentation**. *Missiles and Rockets*, **2** (1), 64–6 (*Jan.*, 1957). Pictures of an exhibition model of the 20-inch Vanguard satellite prototype.

(31) **Lifetimes of artificial satellites of the Earth**. I. G. Henry. *Jet Propulsion*, **27** (1), 21–24, 27 (*Jan.*, 1957). Computes the effect of atmospheric density on the lifetimes of satellites in elliptic and circular orbits, assuming density varies exponentially with altitude. (3 *refs.*)

(32) **Vanguard satellite tracking camera developed**. H. P. Steier. *Missiles and Rockets*, **2** (1), 64–5 (*Jan.*, 1957). Describes a Baker-Nunn camera with 31-inch mirror, focal length of 20 in., and arrangements for satellite tracking and timing.

(33) **Launching platform ready for Vanguard**. *Missiles and Rockets*, **2** (4), 34 (*Apr.*, 1957). Note and picture of this equipment.

(34) **On the powered flight trajectory of an Earth satellite**. B. D. Fried. *Jet Propulsion*, **27** (6), 641–3 (*June*, 1957). Investigates the programme for the powered flight trajectory of an Earth satellite to obtain the maximum orbit altitude. Neglects aerodynamic forces, indicates that optimum programme is to make the tangent of the angle between the thrust vector and the horizontal a linear function of time.

(35) **Soviet astronautics**. A. J. Zaehringer. *Missiles and Rockets*, **2** (2), 45–6 (*Feb.*, 1957). An analysis of published Russian statements on satellite and lunar rocket projects.

36) **The conquest of the Moon**. E. Bergaust. *Missiles and Rockets*, **2** (3), 82–3, 85–6 (*March* 1957). A general description of proposals, both U.S. and Russian, for Moon probe rockets.

(37) **U.S.A.F. to start Moon rocket program**. E. Bergaust. *Missiles and Rockets*, **2** (3), 25 (*March* 1957). Report of projects under way in the U.S. to reach the Moon with rocket probes.

(38) **The possibility of interstellar flight.** J. Gustavson. *Jet Propulsion*, **27** (1), 69–70 (*Jan.*, 1957). A discussion of the problems of propulsion and extreme length of journey time.

(39) **Dynamics of a spinning rocket with varying inertia and applied moment.** K. Jarmolow. *J. Appl. Phys.*, **28**, 308–13 (*March*, 1957). (4 *refs.*)

(40) **Times required for continuous thrust Earth-Moon trips.** D. M. Cole. *Jet Propulsion*, **27**, 416–7 (*April*, 1957).

(41) **The flight path of an electrically propelled spaceship.** E. Stuhlinger. *Jet Propulsion*, **27**, 410–4, 397 (*April*, 1957). Trajectory calculations for non-optimized return trip to Mars.

(42) **Universal gravity turn trajectories.** G. J. Culler and B. T. Fried. *J. Appl. Phys.*, **28**, 672–6 (*June*, 1957). (3 *refs.*)

7—PROPULSION

[*See also* abstract no. 4.]

(43) **One-dimensional inviscid flow through a rocket nozzle.** R. P. Rastogi and T. P. Pandya. *J. Chem. Phys.*, **25**, 1009–12 (*Nov.*, 1956). (5 *refs.*)

(44) **Impulse expressions for rocket systems containing a solid phase.** I. Glassman. *Jet Propulsion*, **27**, 542–3 (*May*, 1957). Use of the standard expression for specific impulse with redefinition of ratio of specific heats and average molecular weight. (3 *refs.*)

(45) **An effect of carbon in an adiabatically expanded gas stream.** S. A. Greene and L. J. Gordon. *Jet Propulsion*, **27** (6), 667 (*June*, 1957). Indicates that carbon particles can trap large percentages of gas and may seriously affect conditions during expansion.

(46) **An approximate specific impulse equation for condensable gas mixtures.** K. A. Wilde. *Jet Propulsion*, **27** (6), 668–9 (*June*, 1957). (3 *refs.*)

(47) **European rocket engineers in Argentine astronautics.** F. C. Durant. *Missiles and Rockets*, **2** (1), 82, 84, 86 (*Jan.*, 1957). An account of the organization and work of the Asociacion Argentina Interplanetaria.

(48) **Approach to solid propellants.** *Missiles and Rockets*, **2** (2), 92 (*Feb.*, 1957). A note pointing out the narrowing gap between the fields of application of solid and liquid fuelled rockets for missile use.

(49) **French solid and liquid rockets.** *Missiles and Rockets*, **2** (2), 82–3 (*Feb.*, 1957). Pictures of aircraft and liquid and solid rocket units.

(50) **Solid-propellant liners.** R. L. Hirsch, C. F. Miller and A. Belin. *U.S. Pat. No.* 2,780,996 (12 *Feb.*, 1957).

(51) **Preliminary photoelastic design data for stresses in rocket grains.** D. D. Ordahl and M. L. Williams. *Jet Propulsion*, **27** (6), 657–662 (*June*, 1957). Photoelastic stress analyses were made on a family of thick-walled cylinders with symmetrical internal slots of differing widths, depths and shape, and in number from 2 to 16. Design charts are presented showing the maximum stresses developed by internal pressure.

(52) **A simple equation for rapid estimation of rocket nozzle convective heat transfer coefficients.** D. R. Bartz. *Jet Propulsion*, **27** (1), 49–51 (*Jan.*, 1957). A paper providing a simple approximation for estimating convective heat transfer coefficients. (9 *refs.*)

(53) **Dynamics of variable-thrust, pump-fed, bipropellant, liquid rocket engine system.** M. R. Gore and J. C. Carroll. *Jet Propulsion*, **27** (1), 35–43 (*Jan.*, 1957). A simulator study of rocket engine systems with results presented for both transient and frequency responses. The effects of system nonlinearities are included. (7 *refs.*)

(54) **New fuel flow test equipment.** A. S. Kevorkian. *Missiles and Rockets*, **2** (1), 60–1 (*Jan.*, 1957). Describes equipment for producing and measuring flow rates of up to several thousand gallons per minute and 1500 p.s.i. for use in rocket motor component testing. Accuracy of one part in 100,000 claimed.

(55) **The Rocketdyne boom.** *Missiles and Rockets*, **2** (4), 73–5 (*April*, 1957). An account of the work and facilities of this firm, responsible for design, development and production of the largest sizes of liquid propellent rocket engines.

(56) **The development of the Armstrong Siddeley 'Snarler' rocket motor.** D. Hurden. *Engineering Designer*, 9–18 (*April*, 1957). Descriptions of problems involved in combustion chamber, injector, pumps, valves, gearbox and flight tests.

(57) **Reliability concepts in a rocket power controls design.** H. L. Coplen. *Jet Propulsion*, **27** (6), 637–40, 643 (*June*, 1957). Presentation of ideas intended to achieve the utmost reliability from a controlled liquid rocket engine.

(58) **Some considerations of film cooling for rocket motors.** M. J. Zucrow and A. R. Graham. *Jet Propulsion*, **27** (6), 650–6 (*June*, 1957). Shows from experimental results that increasing the film-cooled length of a duct requires more than a proportional increase in coolant flow rate. Presents graphs for determining the required flow where the heat transfer rate to the liquid film is known, and data for the loss in specific impulse when water is used as coolant. (28 *refs.*)

(59) **Theoretical performance of some rocket propellants containing nitrogen and oxygen.** R. O. Miller and P. M. Ordin. *N.A.C.A. RM E8A3*, 53 pp. (*May*, 1948).

(60) **Theoretical performance of diborane as a rocket fuel.** V. N. Huff, C. S. Calvert and V. C. Erdmann. *N.A.C.A. RM E8I17a*, 31 pp. (*Jan.*, 1949).

(61) **Theoretical performance of lithium and fluorine as a rocket propellant.** S. Gordon and V. N. Huff. *N.A.C.A. RM E51CO1*, 22 pp. (*May*, 1951).

(62) **Theoretical performance of liquid hydrogen and liquid fluorine as a rocket propellant.** S. Gordon and V. N. Huff. *N.A.C.A. RM E52L11*, 28 pp. (*Feb.*, 1952).

(63) **Theoretical performance of liquid ammonia, hydrazine and mixture of liquid ammonia and hydrazine as fuels with liquid oxygen bifluoride as oxidant for rocket engines. I. Mixture of liquid ammonia and hydrazine.** V. N. Huff and S. Gordon. *N.A.C.A. RM E51L11*, 24 pp. (*Feb.*, 1952).

(64) **Theoretical performance of liquid ammonia, hydrazine and mixture of liquid ammonia and hydrazine as fuels with liquid oxygen bifluoride as oxidant for rocket engines. II. Hydrazine.** V. N. Huff and S. Gordon. *N.A.C.A. RM E52G09*, 20 pp. (*Sept.*, 1952).

(65) **Theoretical performance of liquid ammonia, hydrazine and mixture of liquid ammonia and hydrazine as fuels with liquid oxygen bifluoride as oxidant for rocket engines. III. Liquid ammonia.** V. N. Huff and S. Gordon. *N.A.C.A. RM E52H14*, 15 pp. (*Oct.*, 1952).

(66) **Theoretical performance of liquid ammonia and liquid fluorine as a rocket propellant.** S. Gordon and V. N. Huff. *N.A.C.A. RM E53A26*, 25 pp. (*March*, 1953).

(67) **Theoretical performance of liquid hydrazine and liquid fluorine as a rocket propellant.** S. Gordon and V. N. Huff. *N.A.C.A. RM E53E12*, 83 pp. (*July*, 1953).

(68) **Theoretical performance of mixtures of liquid ammonia and hydrazine as fuel with liquid fluorine as oxidant for rocket engines.** S. Gordon and V. N. Huff. *N.A.C.A. RM E53F08*, 43 pp. (*July*, 1953).

(69) **A method of calculating the performance of liquid propellant systems containing the species C,H,O,N,F and one other halogen with tables of required thermochemical properties to 6000° F. [3317° C.]** J. S. Martinez and G. W. Elverum. *Calif. Inst. Tech. Jet Prop. Lab. Memo.* 20–121, 87 pp. (*Dec.*, 1955).

(70) **Mollier charts for the products of combustion of ammonia and nitric acid.** I. Forsten. *Naval Air Rocket Test Station Rept.* 83, 12 pp. (*April*, 1956).

(71) **Methods of analyzing polysulfide perchlorate propellants.** C. Ribando. *Picatinny Arsenal, Samuel Feltman Ammunition Labs. Tech. Rept.* 2334, 21 pp. (*Sept.*, 1956).

(72) **Theoretical maximum performance of liquid fluorine-liquid oxygen mixtures with JP-4 as rocket propellants**. S. Gordon and R. L. Wilkins. *N.A.C.A. RM E54H09*, 18 pp. (*Oct.*, 1956).

(73) **Ignition delays and fluid properties of several fuels and nitric acid oxidants in temperature range from 70° to −105° F. [21.1° to −76° C.]** R. O. Miller. *N.A.C.A. Tech. Note* 3884, 32 pp. (*Dec.*, 1956).

(74) **Evaluating propellant systems**. J. Irgon. *Missiles and Rockets*, **2** (1), 62–3 (*Jan.*, 1957). Advocates the method of "response surfaces" for analysis and optimization of highly complex systems. (3 *refs.*)

(75) **Ignition of rocket fuels**. G. Traverse. *U.S. Pat. No.* 2,775,863 (1 *Jan.*, 1957). Monoprollents.

(76) **Liquid-hydrocarbon rocket propellants**. D. R. Carmody and A. Zletz. *U.S. Pat. No.* 2,778,188 (22 *Jan.*, 1957).

(77) **Propellant**. J. Cohen and E. D. Besser. *U.S. Pat. No.* 2,779,288 (29 *Jan.*, 1957).

(78) **Case for jelly-propellants**. *Missiles and Rockets*, **2** (2), 84 (*Feb.*, 1957). Note on the possibility of modifying petroleum fuels for use in rocket motors.

(79) **Humidity control for rocket propellants**. *Missiles and Rockets*, **2** (3), 103–4 (*March*, 1957). Describes apparatus for maintaining low water content in materials for manufacture of solid propellent charges.

(80) **Fluorine figures big**. *Jet Propulsion*, **27** (6), 678–9 (*June*, 1957). Comment on current opinion on use of fluorine as propellent.

(81) **Gasoline to kerosene to "zip."** *Jet Propulsion*, **27** (6), 682, 689–90 (*June*, 1957). Review of high-calorific value fuels.

(82) **On the hazards of concentrated hydrogen peroxide**. R. Bloom. *Jet Propulsion*, **27** (6), 666 (*June*, 1957). Claims that the hazards of handling pure hydrogen peroxide have been exaggerated.

(83) **Field transportation of concentrated hydrogen peroxide**. J. H. Keefe and C. W. Raleigh. *Jet Propulsion*, **27** (6), 663–4, 667, 737 (*June*, 1957). Discusses methods of handling hydrogen peroxide commercially and the requirements for tactical service vehicles. (4 *refs.*)

(84) **Chemical aspects of high-energy solid propellants**. A. J. Zaehringer. *Missiles and Rockets*, **2** (6), 110, 112 (*June*, 1957). Requirements of a high-energy solid propellent.

(85) **Nuclear powered rockets and the problems of heat release**. R. H. Reichel. *Missiles and Rockets*, **2** (6), 96–7 (*June*, 1957).

(86) **Ships for space**. P. A. Castruccio. *Missiles and Rockets*, **2** (6), 72–4 (*June*, 1957). Low acceleration techniques and the use of ion rockets.

(87) **Seven league boots**. S. Hull. *Missiles and Rockets*, **2** (6), 63–4 (*June*, 1957). Short review of possible non-combustion propulsion systems.

(88) **The ion rocket**. J. E. Naugle. *Missiles and Rockets*, **2** (6), 87–8 (*June*, 1957). Basic principles.

(89) **Air Force studies ion power**. H. T. Simmons. *Missiles and Rockets*, **2** (6), 76 (*June*, 1957). General review of ion propulsion projects.

(90) **Ion propulsion and why**. E. Stuhlinger. *Missiles and Rockets*, **2** (6), 82, 84–5 (*June*, 1957). Basic principles and comparison with other propulsion systems. (10 *refs.*)

8—MISSILES

(91) **Simulators in Navy missile training**. J. Huson and K. K. Smell. *Missiles and Rockets*, **2** (1), 57–8 (*Jan.*, 1957). Indicates the great advantage of simulators for training purposes, illustrations taken from a Terrier launch simulator.

(92) **Optical aspect system for rockets**. J. E. Kupperian and R. W. Kreplin. *Rev. Sci. Inst.*, **28**, 14–19 (*Jan.*, 1957).

(93) **Too many pushbuttons**. D. E. Mullen. *Missiles and Rockets*, **2** (1), 54–56 (*Jan.*, 1957). A call for greater reliability in missile engineering, particularly through simplicity in design, assembly and testing.

(94) **Navy missiles roles and missions**. J. S. Russell. *Missiles and Rockets*, **2** (1), 38–42 (*Jan.*, 1957). Describes the operational and development stage missiles of interest to the U.S. Navy. The missiles discussed are Sidewinder, Sparrow, Regulus, Terrier, Tartar, Talos, Polaris, Triton, Petrel, Lulu and Dove.

(95) **The surface-to-surface missile today and yesterday**. F. I. Ordway. *Interavia*, **12** (1), 49–51 (*Jan.*, 1957) and **12** (2), 134–7 (*Feb.*, 1957). A review of the U.S. ballistic and aerodynamic missiles.

(96) **$583 Million more for guided missiles**. *Missiles and Rockets*, **2** (2), 22–3 (*Feb.*, 1957). An analysis of the current U.S. defence budget.

(97) **Behind-iron-curtain rocket show**. *Missiles and Rockets*, **2** (2), 74–5 (*Feb.*, 1957). Pictures of exhibits at the observatory in Katowice (Poland).

(98) **Russia's guided missile program**. *Missiles and Rockets*, **2** (2), 33–41 (*Feb.*, 1957). A survey of all known facts on Russian missiles; T1, T2, T3, T4, T4A and T7A long range missiles, T5 and T6 artillery rockets, Comet 1 and Comet 2 submarine-launched long range rocket missile, M100A air-to-air missile and research and upper atmosphere vehicles.

(99) **Soviet missile science profile**. *Missiles and Rockets*, **2** (2), 61–3, 65–7 (*Feb.*, 1957). Report of interviews with scientists having had close contact with Russian scientists.

(100) **Submarine missiles: Reds move ahead**. *Missiles and Rockets*, **2** (2), 54–5 (*Feb.*, 1957). Details of German underwater missile launching platforms and the effect of such devices on modern warfare.

(101) **Rockets change modern army doctrine**. J. B. Medaris. *Missiles and Rockets*, **2** (2), 50–1 (*Feb.*, 1957). A review of the changes in army tactics required to take advantage of the potentialities of missiles.

(102) **What the Russians tell . . . and what they don't tell**. A. Parry. *Missiles and Rockets*, **2** (2), 70–2 (*Feb.*, 1957). Comparison of U.S. opinions and Russian publications on the subject of rocket missiles.

(103) **Tactical reliability, the main test**. P. W. Powers. *Missiles and Rockets*, **2** (3), 89–90 (*March*, 1957). Describes the different ways of assessing reliability as applied to test rockets and rocket weapons, emphasizes that tactical effect at the target should be used for the latter.

(104) **Scientific methods replace "fire-fighting" approach**. H. P. Steier. *Missiles and Rockets*, **2** (3), 65–6 (*March* 1957). Planning missile development for increased reliability is advocated as the best way to achieve rapid results.

(105) **Million-to-one shot**. W. S. West. *Missiles and Rockets*, **2** (3), 93–95 (*March* 1957). Shows the difficulty of high reliability in complex systems, and indicates methods in use to improve it.

(106) **Contacts for contracts**. *Missiles and Rockets*, **2** (4), 70 (*April*, 1957). Presents the branches of the Air Force Office of Scientific Research responsible for research contracts.

(107 **The Ramo-Wooldridge story**. *Missiles and Rockets*, **2** (4), 62–3 (*April*, 1957). An account of a corporation conducting research, development, and production of electronic systems and guided missiles.

(108) **Woomera**. A. V. Cleaver. *Spaceflight*, **1**, 103–6 (*April*, 1957).

(109) **On the optimization of two-stage rockets**. M. Goldsmith. *Jet Propulsion*, **27**, 415–6 (*April*, 1957). (2 *refs.*)

(110) **Up-to-the-minute American missiles**. *Ali*, **9** (8), 21–4 (30 *April*, 1957). (*In Italian.*) Lists 103 missiles with data and uses.

(111) **Rocket propellants for underwater missiles**. A. J. Zaehringer. *Missiles and Rockets*, **2** (5), 91–3 (*May*, 1957). Effect of environment on design of missiles.

(112) **Analogue computing applied to guided weapons**. F. R. J. Spearman. *Discovery*, **18**, 192–7 (*May*, 1957).

(113) **Bending dynamics of a spinning missile**. T. J. Harvey. *Jet Propulsion*, **27** (6), 669–72 (*June*, 1957). (3 *refs.*)

(114) **Soviet rocket launcher revealed.** *Missiles and Rockets*, **2** (2), 28 (*Feb.*, 1957). Description of anti-tank rocket launcher captured at Port Said in 1956.

(115) **U.S. Air Force to begin I.C.B.M. training.** *Missiles and Rockets*, **2** (1), 21 (*Jan.*, 1957). Reports competition between ballistic and aerodynamic missiles for priority in the U.S. defence budget.

(116) **Break-up in Army-Navy Jupiter program.** E. Bergaust. *Missiles and Rockets*, **2** (1), 18–19 (*Jan.*, 1957). Describes schemes for launching intermediate range missiles from submarines.

(117) **U.S.A.F.'s long-range missile program.** *Missiles and Rockets*, **2** (4), 57–61 (*April*, 1957). A history of ballistic rocket missiles and description of the U.S. organizations concerned in research, development, and production of such weapons.

(118) **Who's got what.** *Missiles and Rockets*, **2** (4), 64 (*April*, 1957). List of major contractors and subcontractors in the ballistic missile industry of U.S.A.

(119) **From Redstone to Jupiter.** W. von Braun. *Missiles and Rockets*, **2** (6), 107–8 (*June*, 1957). Design philosophy.

(120) **Churchill firings prove it can be done.** *Jet Propulsion*, **27** (1), 52–3 (*Jan.*, 1957). An account of high altitude rocket firings in October-November, 1956, and notes on the difficulty of arctic conditions.

(121) **The recovery of high speed rocket powered vehicles and/or their components.** R. Provart. *Jet Propulsion*, **27** (2), 125–131, 147 (*Feb.*, 1957). A comparison of different methods and discussion of the characteristics of devices such as metal parachutes, spoilers, divebrakes and retarding rockets. (4 *refs.*)

(122) **1957 Research rocket roundup.** *Missiles and Rockets*, **2** (3), 39–48 (*March*, 1957). Reviews rocket test vehicles used in the U.S. and other countries (not Russia) for high altitude and high velocity experiments. Includes V2, Viking, Wac-Corporal, Aerobee and Aerobee-hi, Deacon, Cajun, DAN, Nike-Cajun, Iris, Arcon, Terrapin, Rockoon, Rockaire, Asp and Wasp; French rockets Veronique and ATEF; and British Skylark.

(123) **Terrapin, the do-it-yourself rocket.** S. F. Singer. *Missiles and Rockets*, **2** (3), 60–2 (*March* 1957). Describes a cheap two-stage rocket vehicle suitable for use in University research programmes. It can carry a 25-lb. load to a height of 80 miles.

(124) **French Monica rocket performs well at low cost.** *Missiles and Rockets*, **2** (5), 50 (*May*, 1957). Three-stage research rocket taking 33-lb. payload to 60 miles at cost of £500. Description and details of seven firings.

(125) **Boost phase trajectory analysis techniques.** C. D. West. *Jet Propulsion*, **27**, 527–33 (*May*, 1957). Discusses four numerical approaches to the solution of the relevant equations for a cruciform, nonguided missile booster. (8 *refs.*)

(126) **Fuel cut-off control for guided missiles.** G. L. Zomber and D. MacMillan. *Electronics*, **29** (1), 126–7 (*Jan.*, 1956).

(127) **Inertial guidance for Air Force high-punch missiles.** J. M. Slater. *Missiles and Rockets*, **2** (4), 77–9 (*April*, 1957). An account of the principles and practical difficulties of inertial guidance systems.

BOOK REVIEWS

Russian-English Glossary of Guided Missile, Rocket, and Satellite Terms. Compiled by Alexander Rosenberg. 10¼ × 7¼ in. Pp. vi + 352. 1958. Washington: U.S. Library of Congress. Reference Dept. ($2.50).

This glossary contains over 4000 entries, the terms being taken from monographs and periodicals published in the U.S.S.R. over the period 1955-58. The English equivalents have been based mainly on Soviet translations of American and British publications. The great advances in astronautics made by Russian scientists and engineers are evidence of the need for Western scientists to keep in touch with such literature on this subject as may be published by the U.S.S.R. (a periodical on rocketry is becoming available to foreign subscribers in 1959), and this glossary will be invaluable to translators and abstractors, and to the growing number of research workers who can at least make their way through scientific texts in Russian though without the ability or inclination to tackle an editorial in *Pravda* or *Izvestiya*

The glossary includes relevant terms from such fringe subjects as meteorology, physics, etc., but the compiler has wisely restrained himself from letting the work degenerate (or evolve? into a general technical dictionary. The excellent *Callaham* still reigns supreme in that field, but needs to be supplemented by glossaries such as this for specialist or rapidly-developing subjects. As Dr. Rosenberg points out in his introduction, the terminology of astronautics is not yet firmly established and new terms and meanings are constantly evolving. He also makes the point that a certain amount of error is inevitable in any work of this kind but can be rectified in future editions with the help of users of the glossary. The reviewer has not made a detailed search for such errors, though he encountered a few typing mistakes (the publication is lithoprinted from typescript), notably "Westcoft" instead of "Westcott," and "Culomb" instead of "Coulomb."

The most serious error occurs in the Table (in the Prelims) giving the transliteration of the Russian alphabet, where "j" has been typed instead of "p." This might be confusing to anyone who has not met Cyrillic before but tries to use the glossary; it would not deter anyone who had taken more than two or three lessons in the language. The transliteration scheme employed is understandable in a Library of Congress publication but is not that in most general use, so it occasionally leads to unfamiliar spelling of names (*e.g.*, the dog Layka).

The paper cover and stapled binding are totally unsuitable for a dictionary such as this, which will receive very heavy usage. Any purchaser should have the book rebound; even with this added expense it will be good value for money.

G. V. E. Thompson.

Tables for Solving the Laplace Equation inside an Ellipse. By A. I. Vzorova. (Translated by L. Herdan and edited by R. Herdan, from ТАБЛИЦЫ ДЛЯ РЕШЕНИЯ УРАВНЕНИЯ ЛАПЛАСС В ЕЛЛИПТИЧЕСКИХ ОБЛАСТЯХ, published by Academy of Sciences U.S.S.R. Moscow, 1957). 11 × 8½ in. Pp. 256. 1958. London: Infosearch Ltd.; distributed by Cleaver-Hume Press Ltd. (50s.).

These tables allow the approximate solution of Laplace's equation $\frac{\partial^2 U}{\partial x^2} + \frac{\partial^2 U}{\partial y^2} = 0$ in the interior of an ellipse $x = a \cos E$; $y = b \sin E$ by expressing the boundary function as a partial sum of Fourier series in the ellipse parameter E and equating a sum of simple harmonic Legendre functions, regular at the origin, to the boundary function at twenty equidistant parametric points of the ellipse. The coefficients of the solution are obtained from Table 1 by a simple row by column multiplication, whilst for greater accuracy Table 2 is available and interpolation is permissible.

B. G. Anderson.

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